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*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

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ON THE LABOURING FORCE OF THE HUMAN HEART

THERE is no organ in our bodies that has a more important influence upon health, at all ages of our lives, than the heart, whose rhythm and force are governed by laws of nerve-force, of which we are at present almost totally ignorant. Regarded, however, from a mechanical point of view, as a hydraulic pumping machine, our knowledge of the heart is more accurate, and may yet lead the way to greater knowledge of the physiological action of this vital organ.

I propose, in the present communication, to give an estimate of the daily labouring force of the human heart, and to compare it with that of other muscles, such as those used in rowing or climbing, reserving for a future communication the proof of the data to be now employed.

The heart, regarded as a pumping machine, consists of two muscular bags (*ventricles*), one of which drives the blood through the lungs, and the other through the entire body. This blood is forced, by a pumping action, repeated seventy-five times each minute, through both lungs and body, and experiences in each case a resistance which is measured by the hydrostatical pressure of the blood in the pulmonary artery and aorta. The resistance offered to the circulation of the blood, by the capillary vessels of the lungs and body, is different; but the total quantity of blood that passes through the lungs and body in a given time, must be the same; from which it follows, that the resistance offered by the capillaries must be in the proportion of the hydrostatical pressure in the great arteries leading from the ventricles of the heart. If, therefore we knew that pressure for one side of the heart, and the relative forces of the two ventricles in contracting, we should know the entire resistance overcome by the heart at each of its beats.

If, in addition to the hydrostatical pressure in one ventricle, and its ratio to that in the other ventricle, we knew also the quantity of blood forced out of each ventricle against this pressure, we should have all the elements necessary to calculate the labouring force of the heart, as will be presently shown.

I demand, therefore, that my reader shall grant me, provisionally, the following postulates, which are necessarily three in number :—

I. That three ounces of blood are driven from each ventricle at each stroke of the heart.

II. That the hydrostatical pressure in the left ventricle and aorta, against which the blood is forced out, amounts to a column of blood 9·923 feet in vertical height.

III. That the muscular force of the left ventricle, in contracting, bears to that of the right ventricle the proportion of 13 to 5.

With these postulates granted, we may now proceed to calculate the daily labouring force of the heart as follows. At every stroke of the heart, three ounces of blood are forced out of the left ventricle against a pressure of a column of blood 9·923 feet in height. The work done, therefore, at each stroke is equivalent to lifting three ounces through 9·923 feet. This work is repeated 75 times in each minute, and there are 60 × 24 minutes in the day. Hence, the daily work of the left ventricle of the human heart is $3 \times 9{\cdot}923 \times 75 \times 60 \times 24$ ounces lifted through one foot; or, since there are 16 ounces in the pound, and 2,240lbs. in the ton, the work done by the left ventricle of the heart in one day is $\frac{3 \times 9{\cdot}923 \times 75 \times 60 \times 24}{16 \times 2{,}240}$ tons lifted through one foot. Multiplying and dividing out this quantity, we find the daily work of the left ventricle is 89·706 foot-tons. The work done by the right ventricle is five-thirteenths of this quantity (post. III.); the daily work of the right ventricle is therefore 34·502 foot-tons. Adding these two quantities together, we find for the total daily work of the human heart 124·208 tons lifted through one foot.

It is not easy for persons unaccustomed to these calculations to appreciate quickly the enormous amount of labouring force denoted by the preceding result; but in order to facilitate this appreciation, I shall compare it with the following descriptions of labour :—

1. The daily labour of a working man.
2. The work done by an oarsman in an eight-oar boat-race.
3. The work done by locomotive engines, or animals climbing a height.

1. The daily labour of a working man, deduced from various kinds of labour, from observations spread over many months, is found to be equivalent to 354 tons lifted through one foot, during the ten hours that usually constitute the day's work. This amount of work is less than three times the work done by a single heart, beating day and night for 24 hours: thus, three old women sitting beside the fire, alternately spinning and sleeping, do more work, by the constant beating of their hearts, than can be done in a day by the youngest and strongest "navvy."

2. If an Oxford eight-oar boat be propelled through the water at the rate of one knot in seven minutes, the resistance offered by the water may be estimated at 81·36 lbs. by calculation, or at 74·15 lbs. by actual observation. From this result, and from the fact that 575 ounces of muscle are employed by each of the eight oarsmen, we can calculate that 15 foot-pounds of work are expended by each ounce of muscle during each minute of work.

No labour that we can undertake is regarded as more severe than that of the muscles employed during a boat-race; and yet this labour, severe as it is, is only three-fourths of that exerted day and night during life by each of our hearts.

The average weight of the human heart, which increases with age (for obvious reasons), may be estimated from the following tables :—

	Average oz.
1. Meckel	10·0
2. Cruveilhier	7·5
3. Bouilland	8·4
4. Lobstein	9·5
5. Boyd (æt. 30—40)	10·4
6. Boyd (æt. 40—50)	10·5
Mean	9·39

From this weight, and the work done by the heart in one day (124 foot-tons), we can calculate the work done by each ounce of the heart in one minute, as follows :—

Work done by the human heart, in foot-pounds per ounce per minute, $\frac{124{\cdot}208 \times 2240}{9{\cdot}39 \times 24 \times 60} = 20{\cdot}576$ foot-pounds.

This amount of work exceeds the work done by the muscles during a boat-race (as already stated) in the proportion of 20 to 15, or of 4 to 3.

3. There is yet another mode of stating the wonderful energy of the human heart. Let us suppose that the heart expends its entire force in lifting its own weight vertically; then the total height through which it could lift itself in one hour is thus found, by reducing the daily work done in foot-tons (124·208) to the hourly work done in foot-ounces, and dividing the result by the weight of the heart in ounces:—

Height through which the human heart could raise its own weight in one hour $= \frac{124\cdot208 \times 2240 \times 16}{24 \times 9\cdot39} = 19754$ ft.

An active pedestrian can climb from Zermatt to the top of Mont Rosa, 9,000 feet, in nine hours; or can lift his own body at the rate of 1,000 feet per hour, which is only one-twentieth part of the energy of the heart.

When the railway was constructed from Trieste to Vienna, a prize was offered for the locomotive Alp engine that could lift its own weight through the greatest height in one hour. The prize locomotive was the "Bavaria," which lifted herself through 2,700 feet in one hour; the greatest feat as yet accomplished on steep gradients. This result, remarkable as it is, reaches only one-eighth part of the energy of the human heart.

From whatever mechanical point of view, therefore, we regard the human heart, it is entitled to be considered as the most wonderful mechanism we are acquainted with. Its energy equals one-third of the total daily force of all the muscles of a strong man; it exceeds by one-third the labour of the muscles in a boat-race, estimated by equal weights of muscle; and it is twenty times the force of the muscles used in climbing, and eight times the force of the most powerful engine invented as yet by the art of man.

No reflecting mind can avoid recognising in its perfection, and regarding with reverential awe, the Divine skill that has constructed it.

SAMUEL HAUGHTON

THE SCIENCE OF LANGUAGE

Darwinism tested by the Science of Language. Translated from the German of Professor August Schleicher, by Dr. Alex. V. W. Bikkers. (London: J. C. Hotten, 1869.)

IT is not very creditable to the students of the Science of Language that there should have been among them so much wrangling as to whether that science is to be treated as one of the natural or as one of the historical sciences. They, if any one, ought to have seen that they were playing with language, or rather that language was playing with them, and that unless a proper definition is first given of what is meant by nature and by natural science, the pleading for and against the admission of the science of language to the circle of the natural sciences may be carried on *ad infinitum*. It is, of course, open to anybody so to define the meaning of nature as to exclude human nature, and so to narrow the sphere of the natural sciences as to leave no place for the science of language. It is possible also so to interpret the meaning of growth that it becomes inapplicable alike to the gradual formation of the earth's crust, and to the slow accumulation of the *humus* of language. Let the definitions of these terms be plainly laid down, and the controversy, if it will not cease at once, will at all events become more fruitful. It will then turn on the legitimate definition of such terms as nature and mind, necessity and free-will, and it will have to be determined by philosophers rather than by scholars.

Unless appearances deceive us, it is not the tendency of modern philosophy to isolate human nature and to separate it by impassable barriers from nature at large, but rather to discover the bridges which lead from one bank to the other, and to lay bare the hidden foundations which, deep beneath the surface, connect the two opposite shores. It is, in fact, easy to see that the old mediæval discussions on necessity and free-will are turning up again in our own time, though slightly disguised, in the discussions on the proper place which man holds in the realm of nature; nay, that the same antinomies have been at the root of the controversy from the days when Greek philosophers maintained that language existed either φύσει or θέσει, to our own days, when scholars range themselves in two hostile camps, claiming for the Science of Language a place either among the physical or the historical branches of knowledge.

It is by supplying a new point of view for the consideration of these world-old problems, that Darwin's book "On the Origin of Species" has exercised an influence far beyond the sphere for which it was originally intended. The two technical terms of "Natural Selection" and "Struggle for Life," which are in reality but two aspects of the same process, are the very categories which were wanted to enable us to grasp by one effort of thought the reciprocal action of the one on the many and of the many on the one; the mutual dependence of individuals, species, and genus; or, from another point of view, the inevitable limitation of spontaneous action by the controlling influences of social life. I may be allowed to repeat what I said on a former occasion:—"Who has thought about the changes which are brought about, apparently by the exertions of individuals, but for the accomplishment of which, nevertheless, individual exertions would seem to be totally unavailing, without feeling the want of a word—that is to say, in reality, of an idea—to comprehend the influence of individuals on the world at large, and of the world at large on individuals; an idea that should explain the failure of Huss in reforming the Church, and the success of Luther; the defeat of Pitt in carrying parliamentary reform, and the success of Russell? How are we to express that historical process in which the individual seems to be a free agent, and yet is the slave of the masses whom he wants to influence; in which the masses seem irresistible, and are yet swayed by the pen of an unknown writer? Or, to descend to smaller matters, how does a poet become popular? How does a new style of art or architecture prevail? How, again, does fashion change?—how does what seemed absurd last year become recognised in this, and what is admired in this become ridiculous in the next season? Or take language itself. How is it that a new word, such as 'to shunt,' or a new pronunciation, such as '*gold*' instead of '*goold*,' is sometimes accepted, while at other times the last words newly coined or newly revived by our best writers are completely ignored or fall dead? We want an idea that is to exclude caprice as well as necessity—that

is, to include individual exertion as well as general co-operation—an idea applicable neither to the unconscious building of bees, nor to the conscious architecture of human beings, yet combining within itself both these operations, and raising them to a new and higher conception. You will guess both the idea and the word, if I add that it is likewise to explain the extinction of fossil kingdoms and the origin of new species:—it is the idea of 'Natural Selection' that was wanted, and being wanted it was found, and being found it was named. It is a new category, a new engine of thought; and if naturalists are proud to affix their names to a new species which they discover, Mr. Darwin may be prouder, for his name will remain affixed to a new idea, to a new genus of thought." *

Professor Schleicher, whose recent death has left a gap in the ranks of the students of language which it will be difficult to fill, has written down the impressions which he, as a comparative philologist, received from a perusal of Mr. Darwin's work, in a letter addressed to his distinguished colleague, Professor Haeckel, of Jena. It is but a slight sketch, and it would not be fair if the English public took the measure of Professor Schleicher's powers from the translation of his pamphlet which has just been published by Dr. Bikkers, under the somewhat inappropriate title of "Darwinism tested by the Science of Language." Professor Schleicher could hardly have thought that the truth or falsehood of Mr. Darwin's theories depended on any test that can be applied to them by the Science of Language. But he thinks rightly that the genesis of species, as explained by Mr. Darwin, receives a striking illustration in the genealogical system of languages, and particularly of the Aryan and Semitic languages; and he very properly calls attention to the fact, that as this ramification of human speech took place within what may be called, if not historical, at least post-tertiary times, it may be useful as a kind of confirmation of Mr. Darwin's theory, which postulates a similar process in far more distant periods of the world's history. "We observe," he says, "during historical periods how species and genera of speech disappear, and how others extend themselves at the expense of the dead. I only remind you, by way of illustration, of the spread of the Indo-Germanic family, and the decay of the American languages. In the earlier times, when languages were still spoken by comparatively weak populations, this dying-out of forms of speech was, no doubt, of much more frequent occurrence, and, as the idioms of a higher organisation must have existed for a very long time, it follows that the pre-historic period in the life of speech must have been a much longer one than that which falls within the limits of historical record. . . . . It is very possible that many more species of speech perished during the course of that time than the number of those which have prolonged their existence up to the present day. This explains the possibility of so great an extension as, for instance, that of the Indo-Germanic, the Finnic, the Malay, and South African families, which, over a large territory, branched off into such a multitude of directions. A similar process is assumed by Mr. Darwin with regard to the animal and vegetable creation; that is, what he calls 'the struggle for life.' A multitude of organic forms had to perish in the struggle in order to make room for comparatively few favoured races."

* "Lectures on the Science of Language." Second Series. Second Edition, p. 309.

Although this struggle for life among separate languages exhibits some analogy with the struggle for life among the more or less favoured species in the animal and vegetable kingdoms, there is this important difference that the defect and the gradual extinction of languages depend frequently on external causes, *i.e.* not on the weakness of the languages themselves, but on the weakness, physical, moral, or political, of those who speak them. A much more striking analogy, therefore, than the struggle for life among separate languages, is the struggle for life among words and grammatical forms which is constantly going on in each language. Here the better, the shorter, the easier forms are constantly gaining the upper hand, and they really owe their success to their own inherent virtue. Here, if anywhere, we can learn that what is called the process of natural selection, is at the same time, from a higher point of view, a process of rational elimination; for what seems at first sight mere accident in the dropping of old and the rising of new words, can be shown in most cases to be due to intelligible and generally valid reasons. Sometimes these reasons are purely phonetic, and those words and forms are seen to prevail which give the least trouble to the organs of pronunciation. At other times the causes are more remote. We see how certain forms of grammar which require little reflection, acquire for that very reason a decided numerical preponderance; become, in fact, what are called regular forms, while the other forms, generally the more primitive and more legitimate, dwindle away to a small minority, and are treated at last as exceptional and irregular. In the so-called dialectic growth of languages we see the struggle for life in full play, and though we cannot in every instance explain the causes of victory and defeat, we still perceive, as a general rule, that those words and those forms carry the day which for the time being seem best to answer their purpose. Why did the French use *maison*, i.e. mansion, for house? Because *casa* having dwindled down to *chez* was not sufficiently distinct in pronunciation, and because *domus* being frequently used for ecclesiastical buildings, was no longer sufficiently precise in its meaning, if applied to an ordinary house. Why do verbs in *ir*, like *finir*, form the plural *nous finissons*, instead of *nous finons?* Because the example which was set in Latin by the early formation of so-called inchoative verbs, like *durescere, florescere, implescere, gemiscere*, proved attractive, partly on account of its removing any doubts on the exact terminations of a verb, partly because of its giving a fuller body to monosyllabic verbs. Thus *finiscere* was substituted for *finire* in all tenses but the infinitive, the perfect, the future, and the conditional; and while this new species, the so-called second conjugation, was gradually being established, a few scattered remnants only survived of the former race, fossilised, petrified, or, as they are called in grammatical parlance, irregular, such as *nous venons* from *venir*, *nous partons* from *partir*, &c.

There is one point on which Professor Schleicher seems to have misapprehended the meaning of Mr. Darwin. According to him, the different species of the Aryan as well as of the Semitic languages presuppose each a typical language from which they are genealogically

derived. There was, according to him, an ancient Aryan language, not only perfect and complete in itself, but so constituted that it contained the germs of everything which we find in Sanskrit, Greek, Latin, German, Celtic, and Slavonic. Such a language may no doubt be constructed theoretically, in the same manner as out of French, Italian, Spanish, and Portuguese, some kind of Latin language might be reconstructed. But such Latin would be very different from real Latin. Historically the admission of type-languages is perfectly impossible. No one would think of deriving the ancient Greek dialects from one actually existing common language containing within itself the germs of every dialect. No one could realise a language which should be at the same time both High and Low, and yet neither High nor Low German. What kind of language could the primitive Celtic have been, if it had to combine the peculiarities of the Gadhelic and the Cymric branches? How could a common Italian language have existed, if it had to maintain and to neutralise the distinctive features of Oscan, Latin, or Umbrian speech? What applies to the dialects of each language, applies with the same force to all these languages in common, when considered themselves as dialects of Aryan speech. As we cannot derive the Greek dialects from a presupposed primitive *κοινή*, we should not attempt to derive the great dialects—viz. Greek, Latin, Celtic, Teutonic, and Slavonic—from a presupposed primitive Palæo-Aryan type of speech. In tracing the origin of species, whether among plants or animals, we do not begin with one perfect type of which all succeeding forms are simply modifications, but we begin with an infinite variety of attempts, out of which by the slow but incessant progress of natural selection, more and more perfect types are gradually elaborated, some of which are still further improved by artificial domestication. It is the same with languages. The natural state of language consists in unlimited dialectic variety, out of which, by incessant weeding, more and more definite forms of languages are selected, till at last by literary cultivation those highly elaborated classical languages are produced which, in spite of their beauty, are nevertheless abnormal and unnatural, and invariably die without leaving any offspring. New languages do not spring from classical parents, but draw their life and vigour from the spoken rustic and vulgar dialects. No reader of Mr. Darwin's books can fail to see that an analogous process pervades the growth of a new species of language, and of new species of animal and vegetable life. But these analogies should not be carried too far. At all events we should never allow ourselves to forget that, if we speak of languages as natural productions, and of the science of language as one of the natural sciences, what we chiefly wish to say is, that languages are not produced by the free-will of individuals, and that if they are works of art, they are works of what may be called a natural or unconscious art—an art in which the individual, though he is the agent, is not a free agent, but checked and governed from the very first breath of speech by the implied co-operation of those to whom his language is addressed, and without whose acceptance language, not being understood, would cease to be language.

There are other spheres of mental activity to which the same remark applies, but to none so much as to language. It might be said, and it has been said by high authorities, that neither in framing his codes of law, nor in settling the rules of morality, nor in believing the truths of religion, is man an entirely free agent, but that the freedom of the individual is necessarily limited by the pressure exercised by all upon all, and by the circumstances and conditions of the age in which we live. It is true, also, that the science of psychology, which forms the basis of juridical, ethical, and religious science, is imperfect unless it has its foundations in physiology. "La tendance de la physiologie moderne," as M. Claude Bernard remarks, "est donc bien caractérisée ; elle veut expliquer les autres phénomènes intellectuels au même titre que tous les autres phénomènes de la vie ; et si elle reconnait avec raison qu'il y a des lacunes plus considérables dans nos connaissances relativement aux mécanismes fonctionnels de l'intelligence, elle n'admet pas pour cela que les mécanismes soient par leur nature ni plus ni moins accessibles à notre investigation que ceux de tous les autres actes vitaux ?"

But in none of these spheres of mental activity is the freedom of the individual so completely absorbed, and all but annihilated, as in the sphere of language. Not only are the first impulses of language purely physical ; not only is the material of language entirely dependent on the physical organs, such as they are ; not only does the activity of the functional nervous centre of speech become quickly habitual, automatic, and almost instinctive, but even in its purely mental aspect, language rests from the very first on an unconscious compromise. Speech in its very nature is mutual : even a mere exclamation is nothing unless it is understood. Even now we do not speak to others as we should speak to ourselves, but speak their language rather than our own. So it was, only in an infinitely higher degree, in the first formation of speech. If we represent the individual speaker by ^{1}, and the unlimited number of his fellow-creatures by x, the conscious freedom of action which can be claimed for any individual speaker may be expressed by $^1/x$, a quantity oscillating between one divided by one, and one divided by infinity. With every generation this x becomes larger and larger, because it includes not only the present, but the more powerful influence of the past, till at last use and habit exercise the power of a tyrant,

"Quem penes arbitrium est et jus et norma loquendi,"

and whose behests we can no more think of disobeying than the laws of nature.

It is but fair to state, in conclusion, that the first suggestion of the necessity of admitting some of the so-called moral sciences to the circle of the natural sciences came, not from the students of psychology and glossology, but from the historian of the inductive sciences, who saw that the old definition of natural science was becoming too narrow, and that with a new definition the circle of physical knowledge had necessarily to be widened. Dr. Whewell wrote in 1845 :—"We have seen that biology leads us to psychology, if we choose to follow the path ; and thus the passage from the material to the immaterial has already unfolded itself at one point ; and we now perceive that there are several large provinces of speculation which concern subjects belonging to man's immaterial nature, and which are governed by the same laws as sciences altogether physical.

It is not our business to dwell on the prospects which our philosophy thus opens to our contemplation; but we may allow ourselves, in this last stage of our pilgrimage among the foundations of the physical sciences, to be cheered and animated by the ray that thus beams upon us, however dimly, from a higher and brighter region."

MAX MÜLLER

THE UNIVERSE

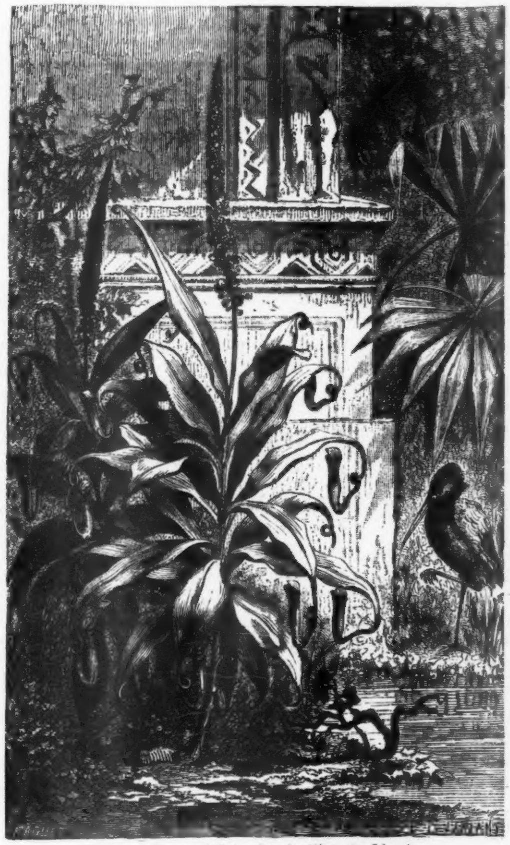
The Universe; or, the Infinitely Great and the Infinitely Little. By F. A. Pouchet, M.D., &c. Pp. 790, 343 engravings, 4 coloured plates. (London: Blackie and Son.)

"WHAT a charming title!" was the thought which first came to us when we saw the announcement of this splendid book. "What a terrible title!" was the

NEPTUNE'S CUP (*Raphidophora latera*)

thought which swiftly followed. Is it a message from some modern prophet to a people, who, having eyes, see not, and having ears, hear not; imploring them to take heed to the tale written in every character in all space, and chanted in every note by every atom, so long and so often in vain? Will it tell us of the signs written in lines of light and lines of black, which have been travelling earthward from the outermost space since the oldest time, till now unnoticed and unread? Will it speak of the oozy mother of living things, which lies and creeps and grows over the whole bottom of the ocean's depths, and comes and goes in every little stagnant pool and slimy puddle? Will it teach us of the quivering flight of atoms in every fire that burns on earth, and in the flaming ministers which rush through illimitable space; of the fairy chains which are welded when the chamber window is sculptured with the frost, and which hold in bonds the elements of the salt that is spilt; and of the giant chains which curb the comets and bind the invisible stars to us? Will it make us to know the great pulsations which shake the earth, and the little throbs which stir the tiny cells of every thing which lives and dies?

All notions of this kind were scattered to the winds when the volume came into our hands. The prophets of



PITCHER PLANT (*Nepenthes distillatoria*, Linn.)

old were clothed in sackcloth and ashes, and those of to-day go about in black, mourning for the sins of the people; but this work is resplendent in purple and gold—a very Dives among books. And every anticipation of a prophetic wail died away when we found that the author was a Frenchman.

It is just such a work as might be expected from a nimble-witted gyrating Gaul, a sort of *petit maitre* of

omniscience. We remember to have heard a criticism passed on a controversial work, made up of many short, somewhat disconnected chapters, to the effect that it reminded the reader of a dog in a kennel coming out at intervals to have a short sharp bark, and then quickly going in again. "The Universe," in a somewhat similar manner, reminds us of the dissolving-views at which the lecturer goes in and out at every view. It is, in fact, a pictorial entertainment, in which M. Pouchet takes the reader, agreeably and without exertion, through all time and all space, with remarks by the way. A picture is presented and the author tells us a little about it, playing all the while (so that the affair may not be tedious) a pretty accompaniment of eloquent diction, charming fancies, and pleasing sarcasm; then another picture is put before the readers, and again another entertainment is begun and finished in like fashion. In this way the author brings before his audience something about most things, treading with a light fantastic mind over the animal and vegetable kingdoms, the formation of the globe, fossils, volcanoes, glaciers, the sun, the stars and immensity, and many other things besides; and as dissolving-views generally end, or used to end, with a chromatrope, so M. Pouchet finishes with an amusing chapter on monsters and superstitions. To every topic there is a picture. We have reproduced two of the smallest and simplest; but a very large number of them are extremely beautiful full-page drawings. And if in them the naked truth is anywhere departed from, it is only for the purpose of heightening the entertainment.

Our readers will already have seen that we regard the letter-press as subsidiary to the pictures; and as far as we can judge, that seems to be also M. Pouchet's own view. The great fault we have to find with the writing, relates to the extreme elegance of the diction. In England we generally talk of bird's nests, but M. Pouchet dwells with zest on the Nuptial Arbour of the Bower Birds; and in the same spirit we have a good deal about "the Nuptials of Plants." In the next edition we shall probably hear something about the Hymeneals of Bathybius.

It is an old question which has puzzled many generations of mothers and nurses, "whether it is better to give a child his powder in jam until he discover the deception, or to be straightforward from the beginning and make the powder go down all nasty as it is." And we may take it for granted that to the general reader simple naked scientific truths are at first as unpalatable as medicine; so that with them too the question of what the old apothecaries used to call "a vehicle" has always to be considered. This question we do not pretend to decide, however strong our own private convictions may be; but to those who range themselves on the side of jam we may recommend this volume as a most skilfully prepared, and not unwholesome confection, with not too much medicine in it. The author states in his preface that he wrote it in the hope of exciting some love of science in his readers, and the researches which have made his name distinguished, are evidence that he has himself a real love of science to no small degree. We can readily imagine how a mind, especially a young mind, fascinated by these beautiful pictures and interested in the lightsome narrative, should let the things grow upon him until there sprang up an actual fondness for plain scientific truth, and he came at last to think that "the medicine was food." To such, and towards such an end, we can heartily commend it.

OUR BOOK SHELF

The Origin of the Seasons. By Samuel Mossman. (Edinburgh: Blackwood & Sons. 1869.)

A PLEASANTLY written and interesting work, spoiled by being coupled with a preposterous theory. Mr. Mossman boldly attempts a difficult task. He proposes to solve a complex problem on very simple principles. Unfortunately his principles are unsound; and overlooking this, there remains the objection that they do not solve his problem. This problem is the well-known fact that in bygone ages plants existed in high latitudes—as far north as England, for example—whose analogues are now only found in the tropics. Mr. Mossman explains this very simply. The obliquity of the ecliptic is now slowly decreasing; therefore it must once have been increasing, and doubtless—though astronomy objects—there was at one time no obliquity: in those days perpetual spring reigned on the earth. But there began a series of upheavals, he says, "directed chiefly towards the northern hemisphere almost exclusively," and this hemisphere becoming overweighted, naturally began to incline. The inclination became at length perhaps twice as great as at present, or even more; but then the southern hemisphere began in its turn to be upheaved, and so checked the increase of inclination, and caused the present process of slow decrease. Mr. Mossman thinks there is nothing in this "contrary to the universal law of gravitation," an opinion which he would modify were he more familiar with that law. The want of balance he speaks of would affect precession and nutation, but not the inclination of the earth's axis. Supposing gravity were on his side, however, and we granted his extension of the tropics, he should remember that the Arctic regions would be equally extended. If he brings the northern tropic to the latitude of London, he has brought the Arctic circle to the latitude of Madrid. Tropical plants in the latitude of Paris, say, would fare ill under this arrangement.

Recherches sur la Faune de Madagascar et de ses Dépendances. I[re] Partie: Relation de Voyage. Par Francois P. L. Pollen. (Leyde: Steenhoff, 1868.)

M. POLLEN, being fond of sport, and having a mind to travel, after consulting Professor Schlegel, started for Madagascar, and spent there a considerable time exploring that and the neighbouring islands, having M. C. Van Dam for companion and preparer of skins, &c. He now publishes the results of his expedition, in large quarto, with profuse illustration in the form of lithographic plates. There are to be five parts to this work—(1) The account of the expedition, (2) the mammifers and birds, (3) the reptiles, (4) the fish, (5) the insects, crustacea, and molluscs. M. Pollen writes the first himself, whilst Professors Schlegel, Bleeker, Vollenhoven, Herklots, and Selys Longchamps assist in the more strictly scientific portion. At present we have only M. Pollen's account of his voyage before us, which is written in a popular style—as he says in the preface—and is as interesting as could be expected. We should suppose that M. Pollen is not himself profoundly scientific; but he has good assistance for the rest of his work.

Country Walks of a Naturalist with his Children. By Rev. W. Houghton. (London: Groombridge and Sons, 1869.)

IF the author had aimed at interesting children of a somewhat larger growth than he has had in view, we think he would have succeeded. The trivial parts of the book will hardly please boys and girls capable of understanding the more solid portions.

A PLEA FOR THE MATHEMATICIAN

II.

I MIGHT go on, were it necessary, piling instance upon instance to prove the paramount importance of the faculty of observation to the process of mathematical discovery.* Were it not unbecoming to dilate on one's personal experience, I could tell a story of almost romantic interest about my own latest researches in a field where Geometry, Algebra, and the Theory of Numbers melt in a surprising manner into one another, like sunset tints or the colours of the dying dolphin, "the last still loveliest" (a sketch of which has just appeared in the Proceedings of the London Mathematical Society),† which would very strikingly illustrate how much observation, divination, induction, experimental trial, and verification—causation, too (if that means, as, if it mean anything, I suppose it must, mounting from phenomena to their reasons or causes of being)—have to do with the work of the mathematician. In the face of these facts, which every analyst can vouch for out of his own knowledge and personal experience, how can it be maintained, in the words of Professor Huxley (who, in this instance, is speaking of the sciences as they are in themselves and without any reference to scholastic discipline), that Mathematics "is that study which knows nothing of observation, nothing of induction, nothing of experiment, nothing of causation"?‡

I, of course, am not so absurd as to contend that the habit of observation of external nature || will be best or at all cultivated by the study of mathematics, leastways as that study is at present conducted; and no one can desire more earnestly than myself to see natural and experimental science introduced into our schools as a primary and indispensable branch of education: I think that that study and mathematical culture should go on hand in hand together, and that they would greatly influence each other for their mutual good. I should rejoice to see mathematics taught with that life and animation which the presence and example of her young and buoyant sister could not fail to impart; short roads preferred to long ones; Euclid honourably shelved or buried "deeper than did ever plummet sound" out of the schoolboy's reach; morphology introduced into the elements of Algebra; projection, correlation, and motion accepted as aids to geometry; the mind of the student quickened and elevated and his faith awakened by early initiation into the ruling ideas of polarity, continuity, infinity, and familiarisation with the doctrine of the imaginary and inconceivable.

It is this living interest in the subject which is so wanting in our traditional and mediæval modes of teaching. In France, Germany, and Italy, everywhere where I have been on the Continent, mind acts direct on mind in a manner unknown to the frozen formality of our academic institutions; schools of thought and centres of real intellectual co-operation exist; the relation of master and pupil is acknowledged as a spiritual and a lifelong tie connecting successive generations of great thinkers in an unbroken chain, just as we read, in the catalogue of our French Exhibition, or of the Salon at Paris, of this man or that being the pupil of one great painter or sculptor and the master of another. When followed out in this spirit, there is no study in the world which brings into more harmonious action all the faculties of the mind than the one of which I stand here as the humble representative and advocate. There is none other which prepares so many agreeable surprises for its followers, more wonderful than the transformation scene of a pantomime, or, like this, seems to raise them, by successive steps of initiation, to higher and higher states of conscious intellectual being.

This accounts, I believe, in part for the extraordinary longevity of all the greatest masters of the Analytical art, the Dii Majores of the mathematical Pantheon. Leibnitz lived to the age of 70; Euler to 76; Lagrange to 77; Laplace to 78; Gauss to 78; Plato, the supposed inventor of the conic sections, who made mathematics his study and delight, who called them the handles or aids to philosophy, the medicine of the soul, and is said never to have let a day go by without inventing some new theorems, lived to 82; Newton, the crown and glory of his race, to 85; Archimedes, the nearest akin, probably, to Newton in genius, to 75, and might have lived on to be 100, for aught we can guess to the contrary, when he was slain by the impatient and ill-mannered sergeant sent to bring him before the Roman General, in the full vigour of his faculties, and in the very act of working out a problem; Pythagoras, in whose school, I believe, the word mathematician (used, however, in a somewhat wider than its present sense) originated, the second founder of geometry, the inventor of the matchless theorem which goes by his name, the precognizer of undoubtedly the miscalled Copernican theory, the discoverer of the regular solids and the musical canon (who stands at the very apex of this pyramid of fame), if we may accept the tradition, after spending 22 years studying in Egypt and 12 in Babylon, opened school when 56 or 57 years old in Magna Græcia, married a young wife when past 60, and died, carrying on his work with energy unspent to the last, at the age of 99. The mathematician lives long and lives young; "the wings of his soul do not early drop off, nor do its pores become clogged with the earthy particles blown from the dusty highways of vulgar life."

Some people have been found to regard all mathematics, after the 47th proposition of Euclid, as a sort of morbid secretion, to be compared only with the pearl said to be generated in the diseased oyster, or, as I have heard it described, "une excroissance maladive de l'esprit humain.

* Newton's Rule (subsequently and for the first time deduced to demonstration in No. 2 of the London Mathematical Society's Proceedings) was to all appearance, and according to the more received opinion, obtained inductively by its author. So also my reduction of Euler's problem of the Virgins (or rather one slightly more general than this) to the form of a question (or, to speak more exactly, a set of questions) in simple partitions was (strangely enough) first obtained by myself inductively, the result communicated to Prof. Cayley, and proved subsequently by each of us independently, and by perfectly distinct methods.

† Under the title of "Outline Trace of the Theory of Reducible Cyclodes."

‡ Induction and analogy are the special characteristics of modern mathematics, in which theorems have given place to theories and no truth is regarded otherwise than as a link in an infinite chain. "Omne exit in infinitum" is their favourite motto and accepted axiom. No mathematician now-a-days sets any store on the discovery of isolated theorems, except as affording hints of an unsuspected new sphere of thought, like meteorites detached from some undiscovered planetary orb of speculation. The form, as well as matter, of mathematical science, as must be the case in any true living organic science, is in a constant state of flux and the position of its centre of gravity is liable to continual change. At different periods in its history, defined with more or less accuracy, as the science of number or quantity, or extension or operation or arrangement, it appears, at present, to be passing through a phase in which the development of the notion of continuity plays the leading part. In exemplification of the generalising tendency of modern mathematics, take so simple a fact as that of two straight lines or two planes being incapable of including "a space." When analysed this statement will be found to resolve itself into the assertion that if two out of the four triads that can be formed with four points lie respectively *in directum*, the same must be true of the remaining two triads; and that if two of the five tetrads that can be formed with five points lie respectively *in plano*, the remaining three tetrads (subject to a certain obvious exception) must each do the same. This at least is one way of arriving at the notion of an unlimited rectilinear and planar scheme of points. The two statements above made, translated into the language of determinants, immediately suggest as their generalised expression my great "Homaloidal Law," which affirms that the vanishing of a certain specifiable number of minor determinants of a given order of any matrix (*i.e.* rectangular array of quantities) implies the simultaneous evanescence of all the rest of that order. I made (*inter alia*) a beautiful application of this law (which is, I believe, recorded in Mr. Spottiswoode's valuable treatise on Determinants, but where besides I know not) to the establishment of the well-known relations, wrung out with so much difficulty by Euler, between the cosines of the nine angles which two sets of rectangular ones in space make with one another. This is done by contriving a matrix such that the six known equations connecting the nine cosines taken both ways in sets of threes shall be expressed by the evanescence of six of its minors; the simultaneous evanescence of the remaining minors given by the Homaloidal Law will then be found to express the Eulerian relations in question, which are thus obtained by a simple process of inspection and reading off, without any labour whatever. The fact that such a law, containing in a latent form so much refined algebra, and capable of such interesting immediate applications, should present itself to the *observation* merely as the extended expression of the ground of the possibility of our most elementary and seemingly intuitive conceptions concerning the right line and plane, has often filled me with amazement to think of.

|| As the prerogative of Natural Science is to cultivate a taste for observation, so that of Mathematics is, almost from the starting point, to stimulate the faculty of invention.

Others find its justification, its "raison d'être," in its being either the torch-bearer leading the way, or the handmaiden holding up the train of Physical Science; and a very clever writer in a recent magazine article, expresses his doubts whether it is, in itself, a more serious pursuit, or more worthy of interesting an intellectual human being, than the study of chess problems or Chinese puzzles.* What is it to us, they say, if the three angles of a triangle are equal to two right angles, or if every even number is, or may be, the sum of two primes,† or if every equation of an odd degree must have a real root? How dull, stale, flat and unprofitable are such and such like announcements! Much more interesting to read an account of a marriage in high life, or the details of an international boat-race. But this is like judging of architecture from being shown some of the brick and mortar, or even a quarried stone of a public building—or of painting from the colours mixed on the palette, or of music by listening to the thin and screechy sounds produced by a bow passed haphazard over the strings of a violin. The world of ideas which it discloses or illuminates, the contemplation of divine beauty and order which it induces, the harmonious connexion of its parts, the infinite hierarchy and absolute evidence of the truths with which mathematical science is concerned, these, and such like, are the surest grounds of its title to human regard, and would remain unimpaired were the plan of the universe unrolled like a map at our feet, and the mind of man qualified to take in the whole scheme of creation at a glance.

In conformity with general usage, I have used the word mathematics in the plural; but I think it would be desirable that this form of word should be reserved for the applications of the science, and that we should use mathematic in the singular number to denote the science itself, in the same way as we speak of logic, rhetoric, or (own sister to algebra‡) music. Time was when all the parts of the subject were dissevered, when algebra, geometry, and arithmetic either lived apart or kept up cold relations of acquaintance confined to occasional calls upon one another; but that is now at an end; they are drawn together and are constantly becoming more and more intimately related and connected by a thousand fresh ties, and we may confidently look forward to a time when they shall form but one body with one soul. Geometry formerly was the chief borrower from arithmetic and algebra, but it has since repaid its obligations with overflowing usury; and if I were asked to name, in one word, the pole-star round which the mathematical firmament revolves, the central idea which pervades as a hidden spirit the whole corpus of mathematical doctrine, I should point to Continuity as contained in our notions of space, and say, It is this, it is this! Space is the Grand *Continuum* from which, as from an inexhaustible reservoir, all the fertilizing ideas of modern analysis are derived; and as Brindley, the engineer, once allowed before a parliamentary committee that, in his opinion, rivers were made to feed navigable canals, I feel sometimes almost tempted to say that one principal reason for the existence of space, or at least one principal function which it discharges, is that of feeding mathematical invention. Everybody knows what a wonderful influence geometry has exercised in the hands of Cauchy, Puiseux, Riemann, and his followers Clebsch, Gordan, and others, over the very form and presentment of the modern calculus, and how it has come to pass that the tracing of curves, which was once to be regarded as a puerile amusement, or at best useful only to the architect or decorator, is now entitled to take rank as a high philosophical exercise, inasmuch as every new curve or surface, or other circumspection of space, is capable of being regarded as the synthesis and embodiment of some specific organised system of continuity.*

* Is it not the same disregard of principles, the same ***indifference to truth for its own sake***, which prompts the question "Where's the good of it?" in reference to speculative science, and "Where's the harm of it?" in reference to white lies and pious frauds? In my own experience I have found that the very same people who delight to put the first question are in the habit of acting upon the denial implied in the second. *Abit in mores incuria.*

† This theorem still awaits proof; it is stated, I believe, in Euler's correspondence with Goldbach: I re-discovered it in ignorance of Euler's having mentioned it, in connection with a theory of my own concerning cubic forms. The evidence in its favour is *induction* of the undemonstrative or purely accumulative kind, and it may or may not turn out eventually to be true. As a most learned scholar who heard this address given at Exeter remarked to me not many days ago, it is certainly by no process of deduction that we make out that five times six is thirty. I mention this, because I know some, who agree, or did agree, with Professor Huxley's published opinions about mathematics, are under the impression that the higher processes of mind in mathematics only concern "the aristocracy of mathematicians:" on the contrary, they lie at the very foundations of the subject. There are besides, and in abundance, mathematical processes which only by a forced interpretation can be brought under the head of demonstration, whether deductive or inductive, and really belong to a sort of artistic and constructive faculty, such for example as evaluating definite integrals, or making out the best way one can the number of distinct branches, and the general character of each branch of a curve from its algebraical equation.

‡ I have elsewhere (in my Trilogy published in the "Philosophical Transactions") referred to the close connection between these two cultures, not merely as having Arithmetic for their common parent, but as similar in their habits and affections. I have called "Music the Algebra of sense, Algebra the Music of the reason; Music the dream, Algebra the waking life—the soul of each the same!"

The early study of Euclid made me a hater of geometry, which I hope may plead my excuse if I have shocked the opinions of any in this room (and I know there are some who rank Euclid as second in sacredness to the Bible alone, and as one of the advanced outposts of the British Constitution) by the tone in which I have previously alluded to it as a school-book; and yet, in spite of this repugnance, which had become a second nature in me whenever I went far enough into any mathematical question, I found I touched, at last, a geometrical bottom; so it was, I may instance, in the purely arithmetrical theory of partitions; so, again, in one of my more recent studies the purely algebraical question of the invariantive criteria of the nature of the roots of an equation of the fifth degree;—the first inquiry landed me in a new theory of polyhedra, the latter found its perfect and only possible complete† solution in the construction of a surface of the ninth order and the sub-division of its infinite contents into three distinct natural regions.‡

Having thus expressed myself at greater length

* M. Camille Jordan's application of Dr. Salmon's Eikosi-heptagram to Abelian functions is one of the most recent instances of this reverse action of geometry on analysis. Mr. Crofton's admirable apparatus of a reticulation with infinitely fine meshes rotated successively through indefinitely small angles, which he applies to obtaining whole families of definite integrals, is another equally striking example of the same phenomenon.

† *Complete* in the sense of ***universal***, more ***than perfect or complete*** in the ordinary sense. Two criteria are absolutely fixed; but in addition to these two an additional criterion or set of criteria must be introduced to make the system of conditions sufficient. The number of such set may be either one or whatever number we please, and into such one or into each of the set (if more than one) an indefinite number of arbitrary parameters (limited) may be introduced. Now the geometrical construction I arrive at contains implicitly the totality of all these infinitely varied forms of criteria, or sets of criteria, and without it, the existence and possibility of such variety in the shape of the solution could never have been anticipated or understood. My truly eminent friend M. Charles Hermite (Membre de l'Institut), with all the efforts of his extraordinary analytical power, and with the knowledge of my results to guide him, has only been able by the non-geometrical method to arrive at one form of solution consisting of a third criterion absolutely definite and destitute of a single variable parameter. As is well known, I have made a very important use of a criterion of the same form as M. Hermite's, but containing one arbitrary parameter (limited). The subject will be found resumed from the point where I left it, and pursued in considerable detail by Prof. Cayley, in one of his more recent memoirs on Quartics in the Philosophical Transactions. M. Hermite it was who first surprised Invariantists (l'Eglise Invariantive, as we are sometimes styled) by an *à priori* demonstration that the nature of the roots or factors of quartics could in general be found by means of invariantive criteria. This was known to be possible up to the *fourth* order of binary quartics, and impossible for the *fourth*. M. Hermite showed that this negation which seemed to stop the way to further progress was an exceptional case; that whereas for the second, third, fifth, sixth, and all higher degrees the thing could be done, for the fourth alone it was impossible: as regards linear quartics, the question does not arise. I look upon this failure of a law for one term in the middle of an infinite progression as an unparalleled ***miracle of arithmetic***, far more real and deeper seated than the one alluded to by Mr. Babbage in connection with the discontinuous action of a supposed machine in his ninth Bridgwater Treatise.

‡ So I found, as a pure matter of observation, that allineation (*alignement*) in ornamental gardening—*i.e.* the method of putting trees in positions to form a very great number or the greatest number possible of straight rows, of which a few special cases only had been previously considered as detached porismatic problems, forms part of a great connected theory of the pluperfect points on a cubic curve, those points, of which the nine points of inflection and Plücker's twenty-seven points serve as the lowest instances.

than I originally intended on the subject, which, as standing first on the muster roll of the Association, and as having been so recently and repeatedly arraigned before the bar of public opinion, is entitled to be heard in its defence (if anywhere) in this place,—having endeavoured to show what it is not, what it is, and what it is probably destined to become, I feel that I must enough and more than enough have trespassed on your forbearance. J. J. SYLVESTER

The remarks on the use of experimental methods in mathematical investigation led to Dr. Jacobi, the eminent physicist of St. Petersburg, who was present at the delivery of the foregoing address, favouring me with the annexed anecdote relative to his illustrious brother, C. G. J. Jacobi *—

"En causant un jour avec mon frère défunt sur la nécessité de contrôler par des expériences réitérées toute observation, même si elle confirme l'hypothèse, il me raconta avoir découvert un jour une loi très-remarquable de la théorie des nombres, dont il ne douta guère qu'elle fût générale. Cependant par un excès de précaution ou plutôt pour faire le superflu, il voulut substituer un chiffre quelconque réel aux termes généraux, chiffre qu'il choisit au hasard, ou, peut-être, par une espèce de divination, car en effet ce chiffre mit sa formule en défaut ; tout autre chiffre qu'il essaya en confirma la généralité. Plus tard il réussit à prouver que le chiffre choisi par lui par hasard, appartenait à un système de chiffres qui faisait la seule exception à la règle.

"Ce fait curieux m'est resté dans la mémoire, mais comme il s'est passé il y a plus d'une trentaine d'années, je ne rappelle plus les détails. "M. H. JACOBI

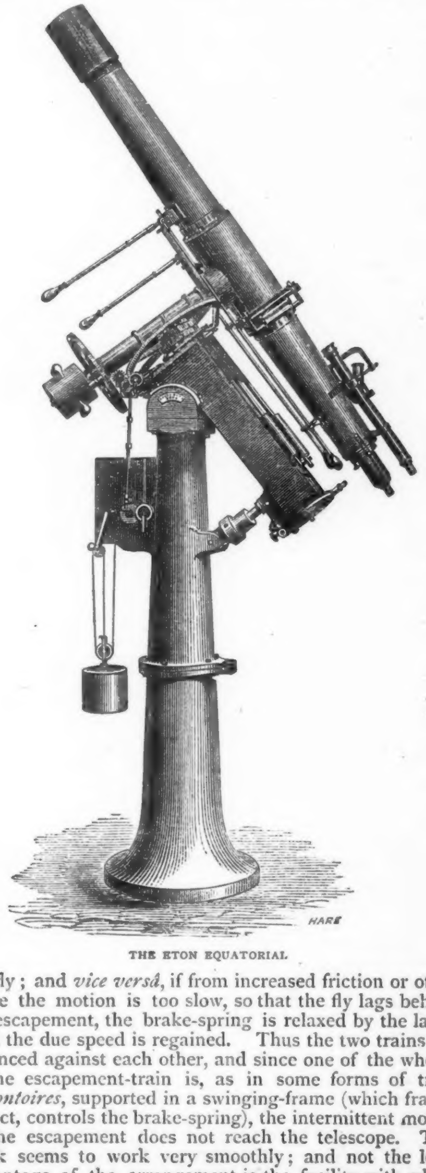
"Exeter, 24 Août, 1869."

THE NEW TELESCOPE AT ETON

IN furtherance of natural science work at Eton, an excellent telescope has been recently given to the school by the energy and liberality of some of the masters.

The instrument is a refractor, with object glass of 5·9 inches clear aperture, and 88 inches focus, and was made by Messrs. Cooke and Sons, of York, who also supplied the observatory and superintended the erection of the telescope. It is, as will be seen from the engraving, mounted equatorially on the German system, with declination circle reading to 10″ of arc, and hour circle reading to 2″ of time. The mechanical details do not, with one exception, deviate materially from the pattern usually adopted by Messrs. Cooke, whose name is a guarantee for skill of design and excellence of workmanship. The exception alluded to is in the construction of the driving clock, the speed of which is not regulated, as usual, by a centrifugal governor, or fly, alone, but by a fly supplemented by an ordinary clock escapement. This arrangement is quite new, and is the invention of the late Mr. T. Cooke, the senior partner in the firm. It was described by him in a paper read before the Royal Astronomical Society a short time ago. The details would hardly be intelligible without drawings, but the general mode of action is as follows :—

The barrel is connected with two trains of wheel-work : one (the lowest wheel of which gives motion in the ordinary way to the telescope) is terminated by a fly of insufficient power *per se* to reduce the speed within proper limits ; the other train is terminated by a half-dead escapement of the usual kind. One of the wheels of the fly-train has a broad rim, on which presses a brake actuated by a wheel in the escapement train. When the escapement is stopped, this brake presses on the wheel with sufficient force to stop the motion of the clock entirely. When the escapement is set to work the brake is released, and the fly-train moves, communicating motion to the telescope. If the speed becomes too great, so as to outrun the escapement, the latter immediately applies increased brake-power, and checks the motion of



THE ETON EQUATORIAL

the fly ; and *vice versâ*, if from increased friction or other cause the motion is too slow, so that the fly lags behind the escapement, the brake-spring is relaxed by the latter until the due speed is regained. Thus the two trains are balanced against each other, and since one of the wheels of the escapement-train is, as in some forms of train *remontoires*, supported in a swinging-frame (which frame, in fact, controls the brake-spring), the intermittent motion of the escapement does not reach the telescope. This clock seems to work very smoothly ; and not the least advantage of the arrangement is the facility with which

* It is said of Jacobi, that he attracted the particular attention and friendship of Böckh, the director of the philological seminary at Berlin, by the zeal and talent he displayed for philology, and only at the end of two years' study at the University, and after a severe mental struggle, was able to make his final choice in favour of mathematics. The relation between these two sciences is not perhaps so remote as may at first sight appear ; and indeed it has often struck me that metamorphosis runs like a golden thread through the most diverse branches of modern intellectual culture, and forms a natural link of connection between subjects in their aims so remote as grammar, philology, ethnology, rational mythology, chemistry, botany, comparative anatomy, physiology, physics, algebra, versification, music, all of which, under the modern point of view, may be regarded as having morphology for their common centre. Even singing, I have been told, the advanced German theorists regard as being strictly a development of recitative, and infer therefrom that no essentially new melodic themes can be invented until a social cataclysm, or the civilisation of some at present barbaric races, shall have created fresh necessities of expression, and called into activity new forms of impassioned declamation.

the speed may be altered from sidereal to lunar rate by merely raising the bob of the pendulum through a small space, so as to diminish the time of oscillation.

For special purposes, still greater accuracy might be obtained if the escapement were worked by the observatory clock by means of a small electro-magnet connected with the pendulum of the latter. Conversely, the escapement train might, with slight modifications and the addition of a dial, be made to serve as a journeyman-clock, and show sidereal time with sufficient accuracy to be very useful in finding stars during two or three hours' work.

There is, by the way, another important modification well worthy of notice. When astronomers wish to determine the position of a star, the diameter of a planet, &c., with rigorous accuracy, they employ a micrometer with spider webs, which in the daytime are visible in the field of view. At night, however, they, or the field itself, require to be lit up. This is managed by a lamp outside and a reflector inside the tube, and to make this lamp perform effectively in every position of the telescope is a difficult matter; so difficult, in fact, on the old arrangement, that Messrs. Cooke and Sons, with their wonderful ingenuity, have entirely superseded it in this their latest instrument. Their exquisite contrivance will be seen from the annexed woodcut.

THE COOKE ILLUMINATING APPARATUS.

L is the lamp. P, a prism to reflect the light on to the tube. D, a disc with diaphrams to regulate the quantity of light. B, a disc with glasses to regulate the colour of the light. S, spring catches to clamp these discs. C, counterpoise of lamp. G, Gravity poise.

The telescope is furnished with a sufficient battery of eye-pieces, of powers ranging from 30 to 400, and also with a bifilar micrometer. The position circle is permanently attached to the lower end of the main tube.

The observatory is erected on the roof of the western tower of the New Schools. It is square, and surmounted by a revolving dome. It is obvious that an instrument erected on a tower cannot be wholly free from vibration; but the latter is reduced to a minimum by supporting the telescope on two massive trussed iron girders stretching across the tower. The floor of the observatory is supported quite independently.

Owing to the unfavourable weather of late, the final adjustments of the telescope have not been completed; but it is hoped that before long it will be in a condition for good and accurate work, such as will justify the enlightened liberality which has placed it where it is.

H. G. MADAN

REMARKS ON TERRESTRIAL MAGNETISM

(Being the substance of a paper read at the Royal Astronomical Society, on Friday, Dec. 10)

SOME years since I was led to the belief that earth currents and auroræ are secondary currents produced by rapid, though small, changes in the earth's magnetism.

In this hypothesis the earth was viewed as similar to the soft iron core of a Ruhmkorff's machine, and the upper and rarer strata of the atmosphere and the moist upper surface of the earth as conductors in which secondary currents would be generated whenever any change took place in the magnetism of the core.

This hypothesis is, I think, confirmed by the very interesting and valuable photographic traces of earth currents obtained by Mr. Airy, at Greenwich, in which, during times of great magnetic disturbance, the earth currents are seen to be very strong, and to vary alternately from positive to negative, lying about equally on both sides of the zero.

It has occurred to me that this method of viewing things is capable of extension, and that it ought to be borne in mind that secondary currents are produced, not only in a *stationary conductor* such as that of the Ruhmkorff's machine, where the magnetic core is made to vary, but also in a conductor which moves in the presence of a magnetic core of constant strength.

Have we not in the earth such conductors in constant motion? We have the return trades constantly proceeding at a high elevation from the equator to the poles, the upper strata of which, from their tenuity, may no doubt be considered to be conductors; in their journey they cross the lines of the earth's magnetic force: ought they not, therefore, to be the vehicles of electrical currents? My friend Mr. Lockyer has lately impressed upon me that the zodiacal light may possibly be a terrestrial phenomenon, and, therefore, that it may be connected in some way with the phenomena of terrestrial magnetism. May it not be the return trades rendered luminous through electric currents in the higher regions of the atmosphere, and may there not also be two species of auroræ, the one occurring in stationary conductors, when the earth's magnetism changes, and the other, in moving conductors, when the earth's magnetism is constant?

But again, it must be allowed that these conductors conveying currents must react on the magnetism of the earth, and we might therefore expect that at those periods of the year at which the system of currents, viewed as meteorological phenomena, change most abruptly, the earth's magnetism would also be particularly liable to change. May not this be an explanation of the excess of magnetic storms about the times of the equinox?

But besides these great terrestrial currents, we have the daily convection currents caused by the sun, which, when they reach the upper regions of the earth's atmosphere, we may imagine to be conductors; and as they also pass across lines of magnetic force, we may suppose them to convey currents. May not these, to some extent at least, account for the diurnal variations of terrestrial magnetism? If this be the case we should have a ready explanation of the likeness observed by Mr. Baxendell between the wind curves and those of the declination.

I have hitherto alluded only to atmospheric currents, but there are also oceanic currents, and more especially there is the tidal wave, which occurs twice every lunar day. No doubt the influence of the tidal wave, as a moving conductor, must be very small; but may it not help to account for the lunar-diurnal variation, which is very small likewise?

But if there is an electric current of this kind in the ocean, it ought to be detected by the system of earth current wires which Mr. Airy has at Greenwich, inasmuch as the surface of the earth and the ocean are in electric communication with each other. Mr. Airy has, if I am not mistaken, detected indications of lunar-diurnal inequalities in the results of his observations. On the other hand, he has detected no current with a single daily period that would account for the diurnal variation—a result in accordance with these views, since the currents producing such would be in the upper regions of the atmosphere.

These views are given in order to invite criticism and

comment; and they will have served their purpose if they direct attention to the part played in the phenomena of terrestrial magnetism by moving conductors. It will be noticed that they leave untouched the mysterious and important connection between sun spots and magnetic disturbances. B. STEWART

P.S.—Since writing the above, Prof. Sir W. Thomson has informed me that Faraday tried to detect induction currents by tides in the Thames, but found no positive result. In an article in the *Philosophical Magazine*, Dec. 1851, Prof. Sir W. Thomson quotes this idea of Faraday, and makes a proposal to test it from tides in the Channel. He also discusses the part which may be played in the phenomena of terrestrial magnetism by moving conductors.

BRITISH RAINFALL

MR. G. J. SYMONS (62, Camden Square, N.W., December 22, 1869) sends us the following list of localities whence observations are "urgently required;" we think he will have many volunteers:—Cornwall: Falmouth, Jacobstow. Devon: Hatherleigh, Hartland, Exmoor. Dorset: Bere Regis. Oxford: Thame. Surrey: Redhill. Suffolk: Halesworth, Mildenhall. Lincoln: Kirton. Nottingham: Mansfield. Warwick: Stratford-upon-Avon. Shropshire: Bishop's Castle. Yorkshire: Milford Junction, Pateley Bridge, Kettlewell, Askrigg, Driffield, Bridlington, on the North York Moors. Lancashire: Broughton. Northumberland: Haltwhistle. Cumberland: Kirkoswald. Westmoreland: Ravenstonedale. Isle of Man: any part of the Island. Pembroke: Tenby, Fishguard. Cardigan: Aberaeron, Brecknock, Llanwrtyd. Radnor: Builth, Knighton. Montgomery: Montgomery, Llanfyllin. Merioneth: Barmouth, Harlech. Carnarvon: Pentrevoclas. Wigtown: Northern part of. Kirkcudbright: Western part of. Peebles: Peebles, Biggar. Ayr: Muirkirk. Argyle: Mull of Cantire, Ballimore, Glencoe, near Ben Cruachan. Perth: North-west part of. Forfar: Western part of. Inverness: along the Caledonian Canal, and in Lochaber. Aberdeen: North-west part of. Nairn: any part of. Banff: Southern part of. Ross: any place inland, or in Lewis or Harris. Sutherland: any place inland. Caithness: any place inland. Ireland: except from the vicinity of Belfast, Dublin, Londonderry, and Waterford, where there are many observers, returns are required from nearly all parts of the country.

THE LATE PROFESSOR MICHAEL SARS, OF CHRISTIANIA

THIS eminent zoologist died on the 22nd of October last; and his loss will be much felt by all naturalists who have benefited, as I have done, by his long, laborious, and conscientious investigation of the invertebrate fauna of the Norwegian seas.

He was born on the 30th of August, 1805, at Bergen, where his father was a shipowner. After finishing his academical studies at Christiania, and evincing at an early age his predilection for natural science, he entered into priest's orders, and in 1830 became pastor at Kinn, in the diocese of Bergen. Ten years afterwards he had charge of the parish of Manger in the same diocese. As both these parishes were on the sea-coast, Sars had constant opportunities of pursuing his zoological researches. In 1829 he published his first essay, entitled "Bidrag til Söedyrenes Natur-historie," and in 1846 the first part of his celebrated work "Fauna littoralis Norvegiæ." In 1854 he was appointed Professor Extraordinarius of Zoology at the University of Christiania, a position which he filled up to the time of his lamented death with great honour to his country, and to the satisfaction of the whole world of science. His celebrity as a zoologist, as well as a palæontologist, was fully recognised by all naturalists and geologists, and he was elected a member of several foreign scientific societies. Our own distinguished countryman, the late Edward Forbes, individually showed his appreciation of Sars's labours in eloquent pages (66 and 67) of his own posthumous work, "The Natural History of the European Seas," when he said, "More complete or more valuable zoological researches than those of Sars have rarely been contributed to the science of Natural History, and the success with which he has prosecuted investigations claiming not only a high systematic value, but also a deep physiological import, is a wonderful evidence of the abundance of intellectual resources which genius can develop, however secluded and wherever its lot be cast;" and he added that the name of this Norwegian priest, "who reaped reputation when seeking no more than knowledge familiar to every naturalist in Europe and America, in Asia, and at the Antipodes—for there are great naturalists settled far in the south, and many in the far east—is a sufficient proof that able work brings the rewards of applause and veneration, even when they be unasked for." By the observations of Sars on the development of the Medusæ he greatly advanced our knowledge of that remarkable physiological phenomenon known as the alternation of generations, which Chamisso had first indicated in the Salpæ. His last publication, "Mémoire pour servir à la connaissance des Crinoïdes vivants," caused especial interest, by showing that a race of animals, supposed to be extinct for a period so long as only to be measured by the duration of several past geological epochs, occurred in a living state in the abysses of the Norwegian seas. This discovery mainly induced the recent exploration of our own seas at great depths, which has produced such wonderful results; and the living Crinoid, or "stone-lily" (*Rhizocrinus Lofotensis*), has now been ascertained to inhabit many parts of the Atlantic from the Loffoden Isles to the Gulf of Mexico. The published works of Sars are seventy-four, and they are not less sound and valuable than numerous. One of his sons, Dr. George Ossian Sars, inherits the zoological inclinations and talent of the late Professor, and is second to none in the knowledge of the Sessile-eyed Crustacea.

It is exceedingly to be regretted that, in spite of the most rigid economy, the large family of Professor Sars is left in very impoverished circumstances, six of his children being wholly unprovided for. May I hope that naturalists and lovers of science will assist me in making a subscription for the temporary relief of this distressed family, and that they will by such tribute to his memory express their admiration of his career and services? I shall be very glad to receive any contributions.

J. GWYN JEFFREYS

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Cuckows' Eggs

I AM very grateful to Mr. Sterland for asking for further information "on some points of difficulty" in my recent paper on Cuckows' Eggs, because it shows me where I have failed in making myself plainly understood. In endeavouring to reply so far as lies in me to his questions, I will take them in order.

1. "Are they [Cuckows' Eggs] so variable as some assert?"

Mr. Sterland supports the doubt here indicated by the statement of "one of the most eminent and experienced of living oologists;" but who this oologist may be he leaves to be guessed, and I venture with all respect to remark, that quoting an anonymous authority in natural history is quoting no authority at all. I am therefore not willing to bring such experience as I myself have had into conflict with that of this eminent but nameless person. Still, as Mr. Sterland is not satisfied with the opinion on this

point of the German authors* cited by Dr. Baldamus, I will here adduce the following passages :—

"Quant au genre Coucou (*Cuculus*) et notamment à l'espèce type du genre, notre Coucou Chanteur (*C. canorus*), on sait quelle étonnante diversité offre la coloration de son œuf, toujours de forme ovée, diversité telle que nous nous abstiendrons d'en aborder la description détaillée."—DES MURS, *Traité Général d'Oologie Ornithologique*, &c. Paris : 1860, p. 219.

"Ces œufs sont très-petits relativement à la taille de l'oiseau, et varient beaucoup pour la couleur. Ils sont ou cendrés, ou roussâtres, ou verdâtres, ou bleuâtres avec des taches petites et grandes, rares ou nombreuses, d'un cendré foncé, vineuses, olivâtres ou brunes, avec quelques points et parfois des traits déliés noirâtres. Nous en possédons deux du blanc le plus pur, et un autre d'une seule teinte bleu-verdâtre, pris dans un nid de Stapazin."—DEGLAND et GERBE, *Ornithologie Européenne*, &c. Paris : 1867, vol. i. p. 163.

I produce this testimony as to *facts* with the greater confidence, because the *opinions* of the witnesses differ from my own, and not one of them, so far as I can gather from their works, was acquainted with Dr. Baldamus's essay.

2 and 3. "Were these [sixteen varieties of eggs] seen to be deposited by the bird, or how were they identified as those of the cuckow? . . . . Is there not room for error here?"

The evidence on which the eggs in question were referred to the Cuckow has been printed in full by Dr. Baldamus and the translator of his essay. To repeat it here would occupy much space and, I think, be unnecessary. It is of much the same kind as the evidence with regard to most Cuckows' eggs. I will freely grant that it might be more satisfactory—if it were so my former paper would never have been written, since naturalists must then have at once accepted the theory. But, on the other hand, I have a right to ask this : If the eggs in question were not Cuckows', what birds laid them? Surely not those in whose nests they were found, because it is a fact which most oologists will confirm, that when birds lay larger eggs than usual the colouring is commonly less deep, and though exceptions may occasionally be found, yet here we have sixteen which are at the same time larger than usual, and of a colour at least as deep, supposing them to belong to the nest-owners. *Sixteen* cases are too many to be exceptional, but this is the number only of the specimens *figured* by Dr. Baldamus; upwards of *sixty* are more or less fully *described* by him.

4. "How then is this process effected?"

In answer to this, Mr. Sterland quotes a very brief summary of my own explanation, to which I have nothing now to add.

5 to 9. The next five questions, for brevity's sake, I will not repeat. They are very pertinent, but are far more easily asked than answered, for they open a wide field of speculation and investigation, since all the hitherto unexplained phenomena of "Dimorphism," "Trimorphism," and "Polymorphism," seem to enter here. But with respect to one of the questions (No. 6), I submit that even if there were no other instance satisfying the conditions imposed by Mr. Sterland than that which I alleged, it is no true argument against the truth of what I advanced. But I think there is an indication of it in other species bearing very directly on the point. Take the Blackcap Warbler and the Tree-Pipit. The eggs of the first are well known to present at least two very different appearances, and those of the second are still more variable. Since Mr. Sterland will not allow that my Eagles fulfil his conditions (and of course he has a perfect right to do so), perhaps he will permit me to bring forward these birds. I have some reason for believing that the same hen Blackcap constantly lays eggs of similar colour. Do the birds of this species hatched from eggs with reddish shells lay eggs of the same character, or brownish ones, and *vice versâ?* If of the same character, we have such an example as is required. If of the other colour, it becomes a case in some measure of "Alternate Generation," but still reducible to a law. That there should be no law at all seems to me at least unlikely, though I fear its discovery is hard.

Certain facts of Dimorphism and Polymorphism are known, but I have not met with any attempted explanation of the phenomena even in such decided and remarkable cases as those of the Malayan Butterflies given by Mr. Wallace (Trans. Linn. Soc. vol. xxv. pp. 5-11). Why the different forms of one species of *Papilio* inhabiting the same district remain distinct is perhaps more unaccountable than that the different forms of Cuckows' eggs should be preserved, for it does not seem to me unlikely that the colour of the egg and the maternal instincts should depend upon the *hen* bird; in which case, granting the hereditariness (if I may make such a word) of the qualities already specified, I think there would be no difficulty.

10. A full reply to Mr. Sterland's last question would lead me to anticipate much that I intend to say when you again permit me to trespass on your readers' forbearance. Consequently, I must defer it until I come to the consideration of "Cuckows' Dupes." ALFRED NEWTON

Cambridge, Dec. 11, 1869

By way of postscript of my letter of the 11th of December (for the delay in publication of which I am in no way accountable*), permit me to offer a few remarks on the communications of Mr. Dresser and Mr. Cecil Smith which have since appeared.

Mr. Dresser says (p. 218) that he "cannot quite agree with Professor Newton that Cuckows' eggs *as a rule* are subject to great variety." I am not aware that I had made such an assertion. The nearest approach to it that I can find is my statement (p. 74), that "it has long been notorious to oologists that eggs of the Cuckow (*i.e.* of the Common Cuckow of Europe—the only species I had mentioned) are subject to very great variety," and in proof thereof I have since furnished some other (and, I think, satisfactory) evidence. Mr. Dresser himself has also brought two or three additional examples which confirm my statement. For the knowledge of these I am much obliged to him, as well as for stating the result of his own experience in support of my supposition that the eggs of the same hen Cuckow resemble each other.

Mr. Cecil Smith (p. 242) seems to me to be as unfortunate in his interpretation of my remarks as he was in that of Dr. Baldamus's (p. 75, note). I feel sure that I have not "pruned and paved" down the doctor's theory so "that there is but little of the original left." To the facts alleged by that naturalist I have taken no exception—on the contrary, I have borne witness (pp. 74, 75) to their general truth; and in the attempt to offer a reasonable explanation of them, I am certain that my "manipulation" is not open to any such charge as that made by Mr. Smith. My "cautious and limited statement" is not different from that of the doctor, nor does "it entirely sweep away" a single assertion of his as to matters of fact. Mr. Smith, apparently, thinks because I have referred to the number of Cuckows' eggs yearly found in nests of the Hedge-Sparrow in this country, without ever bearing any resemblance to the eggs of that bird—a fact, of course, fully admitted by him—that I must thereby deny the single exceptional case adduced from Germany by Dr. Baldamus; but I have never maintained, because no likeness is to be traced in a great many instances, that none was ever perceptible, and accordingly there is no "issue of fact" between the doctor and myself. I must take the liberty of adding, that Mr. Smith, having, as I before showed, misunderstood Dr. Baldamus, has now misunderstood me; and this being the case, it is perhaps needless for me to take up more of your space. ALFRED NEWTON

January 3, 1870

The Veined Structure of Glaciers

I THINK there is no one point in connection with glaciers more interesting than their veined structure, or one upon which so much has been written that remains equally unsettled. The difference of opinion about it between the authors who have published most upon the subject are not less remarkable than the phenomenon itself: no two are agreed, except in considering it as a constitutional feature.

Professor Agassiz maintains (*Atlantic Monthly*, Dec. 1863) that the horizontal layers of pure ice which are formed between the beds of snow from which a glacier is born, constitute many of the identical veins or plates of pure ice which pervade the glacier when it is in full life and activity; and attributes the inclination which they make, in the latter case, to their former horizontal position, to the contortion, bending, or folding, to

* They are Naumann, Thienemann, Brehm, Gloger, and Von Homeyer. Unfortunately, Dr. Baldamus does not refer to the passages in their writings wherein this opinion is expressed; and as most of these writings are somewhat voluminous, I have not always been able to find what are the passages meant. I presume that Mr. Sterland has been more fortunate, for he would scarcely doubt the assertions without knowing what they were, and I should be much indebted to him if he will tell me where they occur—indeed, I am uncertain which of the Brehms and which of the Von Homeyers is intended.

* [The delay in the publication of Prof. Newton's letter is owing to an oversight. It was received prior to the communications of Mr. Dresser and Mr. Cecil Smith, printed in our eighth and ninth numbers.—ED.]

which they have been subjected on their downward course; but, at the same time, he distinguishes between these veins—the result of stratification, and others which he terms bands of infiltration, and which he believes to have been formed by the infiltration and freezing of water.

The late Principal J. D. Forbes maintained ("Occasional Papers on the Theory of Glaciers," 13th letter) that the veins of stratification were annihilated at a certain point, and that at precisely the same time other veins, approximately at right angles to the former ones, were formed. These effects he referred to intense pressure.

Professor Tyndall ("Glaciers of the Alps," pp. 380, 425-6), agrees with Professor Forbes "in ascribing to the structure a different origin from stratification," and, if I understand him rightly, does not believe that *any* portion of the (approximately) vertical veins have such an origin. He divides the veins into marginal, transverse, and longitudinal structure, and asserts that all are produced by pressure, which causes partial liquefaction of the ice, and that the water is refrozen when the pressure is relieved.

If any one cause produced the whole of the veins of pure ice that are found in the imperfect ice of glaciers (which all are agreed are a constitutional feature of those bodies), it is obvious that that cause would have to be equally generally distributed. It is indisputable that all the veins are not veins of stratification, because examples have been frequently observed crossing (cutting) the strata lines at a larger or smaller angle. But although such observations prove conclusively that all the veins must not be attributed to stratification, they do not prove any more. I believe, with Professor Agassiz, for reasons advanced elsewhere,* it can be demonstrated, equally conclusively, that many of the veins which are seen in the lower courses of glaciers in the Alps are veins originally produced by stratification, and dissent entirely from the "annihilation" of Principal Forbes. But as it is proved that some have a different origin, we must look to other causes for an explanation. It is probable that the theories quoted above offer a practical solution of the difficulty, although they are unfortified by direct proofs. But I have seen examples which it was difficult to explain by either one or the other.

There is one means by which the veins might be produced, which, if not overlooked, is at least not generally advanced. All glaciers have crevasses; a glacier is known by its crevasses. The sides of all crevasses become more or less weathered and coated with a glaze of pure ice. When they close up again, when the sides join by virtue of regelation, does this leave no trace? Can it be annihilated? Or, do the two coalesced films leave their mark as a vein of pure ice throughout the generally whitish mass of the glacier? I consider a large number of the veins of pure ice which constitute the "veined structure" of glaciers as nothing more than the *scars of healed crevasses.*

It is not easy to say whether this was the meaning of the following passage, taken from p. 201 of Forbes's "Occasional Papers:" "Most evidently, also, the icy structure is first induced near the sides of the glacier where the pressure and working of the interior of the ice, accompanied with intense friction, comes into play, and the multitudinous incipient fissures occasioned by the intense strain are reunited by the simple effects of time and cohesion." Judged by his preceding pages, it is not, and I am unaware that it has been, advanced in any other place. Some of your readers may perhaps be able to throw some light upon the subject.

Dec. 13, 1869 EDWARD WHYMPER

Irish Lepidoptera

IN reply to the note appended to the report of the Dublin Natural History Society's Meeting, Dec. 1st (NATURE, No. 6, p. 176), allow me to say that I perfectly remember the specimen of *Liminitis* to which Mr. Andrews refers, and which he exhibited some years ago as *Liminitis Sibylla* from Tarbert in the county of Limerick. It was subsequently given by him to my friend Mr. A. Dunlop, of Sutton, near Dublin, and sent to me for identification.

I examined it carefully, and it is a specimen of *Liminitis Camilla*, and *of Continental origin.* How it came to be mixed with Mr. Andrews's Irish specimens remains for him to explain.

To say that the insect is neither Sibylla nor Camilla is absurd; these are the only two species of the genus which inhabit Europe; and Camilla, to which Mr. Andrews's specimen belongs, is the most unlikely of the two to occur in Ireland, as Sybilla is found in England, but not Camilla. However, the insect is in Mr. Dunlop's Collection, and I am sure that gentleman will have pleasure in allowing anyone to inspect it.

As to *Chrysophanus Virgaurea*, there is no British specimen known, nor is there any trustworthy record of the capture of the species in the British Islands.

Chrysophanus Hippothoe (variety *Dispar*) was formerly taken in profusion in the fen districts of Cambridge and Huntingdon, but not that I am aware of in any other part of England. It has been extinct for many years as a British insect, and there is no record of its capture in Ireland at any time. Mr. Andrews's statement that he "met with" Dispar in Kerry is indefinite. Did he capture it? or did he only see it, or suppose he saw it? The most experienced collector may mistake an insect on the wing; and delighted as entomologists would be to welcome back the long-lost Hippothoe, they will require very different proof of its reappearance to any which has yet been adduced.

EDWIN BIRCHALL

Airedale Cliff, Newlay, near Leeds, Dec. 20, 1869

Deep Sea Corals

IN the postscript to Mr. J. Gwyn Jeffreys's report on the "Deep-sea Dredging Expedition in H.M.S. *Porcupine*," I notice the following sentence:—"The presence of corals at great depth will also materially alter the views generally received of the depth at which reef-builders may work, and modify to a certain extent Darwin's theory of the reefs and their mode of growth." This opinion has gained much credence, but it is founded upon error, and is a mistake. Count Pourtales has been good enough to send me the commonest corals which he dredged up off Florida and the Havana from depths greater than 100 fathoms. He has forwarded also the description of the species, and a note upon the nature of the *genera* represented in the depths of the Gulf of Mexico, and which have not been as yet described. I have received the greater part of the corals dredged up during the expedition in the *Porcupine*, and have examined the specimens carefully. Being thus acquainted with the deep-sea coral fauna of both sides of the Atlantic, and having a previous knowledge of the species of the Mediterranean, I have no hesitation in asserting that there is not one species found in these deep seas which is "reef-building" in its habit or whose structures resemble those of the true reef forms. Mr. Darwin's theory is therefore as yet as strong as ever.

Dec. 23, 1869 P. MARTIN DUNCAN

A Meteor

MY attention has just been called to an error in my letter of November 6, which appeared in NATURE, p. 58, respecting the meteor of that evening. I refer to the statement that, of the meteor-cloud, the "longest axis was in the line from the north-west point of the horizon to the pole-star." Instead of *north-west*, it should have been *south-west*, or, perhaps, more correctly, S.W. by W. WM. PENGELLY

Lamorna, Torquay, Dec. 31, 1869

NOTES

THE trigonometrical survey of England and Wales, on the scale of one inch to a mile, has been completed during the past week. It was commenced in the year 1791.

TELEGRAPHIC connection with Australia is about to be carried out by the British-Australian Telegraph Company. The work will consist of 563 miles of cable from Singapore to Batavia, and will join the Dutch line which crosses to the south-eastern extremity of Java, from which point another cable of 1,163 miles is to be laid to Port Darwin. A land line of about 800 miles will connect this with all the Australian colonies. From England to Singapore the messages will be taken by the Falmouth and Malta, the Anglo-Mediterranean, and the British-Indian Extension Companies; thus forming a complete route.

THE eighth part of Wurtz's *Dictionnaire de Chimie* has just been issued. The *Revue des Cours Scientifiques*, to which we are indebted for this announcement, calls particular attention to the articles *Eaux* and *Composés diazoïques* by M. Gautier, *Dissociation* by M. H. Debray, and *Engrais* by M. Dehérain.

THE Agricultural Academy of Poppelsdorf, near Bonn, has recently sustained a severe loss in the death of its able and ener-

* British Association, 1866 (Nottingham).

getic director. Dr. Edward Hartstein was not only a man of wide scientific attainments, but a good practical farmer, and thoroughly acquainted with the English and Scotch systems of agriculture in their more advanced phases. Appointed to the directorship of the Poppelsdorf Academy at the early age of 32, he devoted himself heart and soul to the interests of that institution, and to the advancement of agriculture in his native country. He was the author of various works on rural economy, and for some time immediately previous to his death had been busily engaged in working up the materials for a book which should combine the results of his practical experience with the most recent conclusions of the botanist, the chemist, and the physiologist. Dr. Hartstein was born in 1823, and died on the 14th ultimo, a victim to over-work. He was a member of the Royal British Agricultural Society, a distinction which he highly prized.

THE *Engineer* states that a meeting of Government science teachers was held in Manchester on Monday week, when it was resolved to submit to the Department of Science and Art an expression of regret and disapproval of the sudden and unanticipated mode in which that department has repudiated its engagements with the teachers, chiefly with reference to lectures. The received theory that public companies have no conscience seems to be very generally true of public departments, except when they happen to be dealing with interests strongly represented in the House of Commons.

M. LORTET, Professor at the School of Medicine in Lyons, has undertaken the Natural History Course at the Faculty of Sciences in that city, vice M. Jourdan, who retires.

THE Council of the Society of Arts have decided to create a new office, that of Inspector of the Educational Department, and have selected for this appointment Mr. Critchett, who has been for thirteen years Assistant-Secretary. The latter office will not be filled up.

THE series of public lectures on scientific subjects which is given every winter at the Sorbonne commenced on the 23rd ultimo, when M. Fernet discoursed on the subject of optical illusions. The other lectures comprised in this course are as follows:—Jan. 6.—M. Garnier, Mining Engineer, "The Island of Otaheite." Jan. 13.—M. A. Cazin, "The Motor Forces." Jan. 20.—"M. P. Bert, Professor at the Faculty of Sciences of Paris, "Sympathetic Nervous Actions." Jan. 27.—M. Lies-Bodard, Professor at the Faculty of Sciences at Strasbourg, "Ozone." Feb. 3.—M. Janin (of the Institute), "Sound and Light." Feb. 10.—M. Wolf (of the Imperial Observatory), "The Shape of the Earth." Feb. 17.—M. Jansen, "The Eclipse of the 18th August observed in the East Indies." Feb. 24.—M. Bouley (of the Institute), "On Insanity." March 10.—M. Faye (of the Institute), "On the Figure of Comets." March 17.—M. G. Ville, Professor at the Museum of Natural History in Paris, "Theoretical Agriculture." When shall we be able to announce a like series of lectures by men of scientific eminence, as open free to all comers in our own metropolis?

A COMMITTEE has been formed at Leipzig to collect funds for the purchase of the celebrated museum of the late Dr. Klemm, of Dresden. This museum consists of some 14,000 admirably arranged objects, illustrative of what is known in Germany as the history of civilisation. Should the committee succeed in raising sufficient money to attain their purpose, the collection will be handed over to the University of Leipzig, on condition that it is made available for all classes of society. In consideration of the scientific importance of keeping together such a collection as this, the representatives of Dr. Klemm are willing to sell it to the committee for the moderate sum of 10,000 thalers.

THE first number of the *Annales des Sciences Geologiques*, a periodical on the same plan and of the same size as the *Annales des Sciences Naturelles*, has just been issued. M. Hébert, Professor of Geology at the Faculty of Sciences of Paris, is the geological editor; M. Alphonse Milne-Edwards undertakes the palæontology. One volume, consisting of four numbers, will be published annually. The greater portion of the number before us is occupied by the first part of an important treatise by M. Louis Lartet, secretary of the French Geological Society, "On the Geology of Palestine and the neighbouring countries." This treatise embodies the observations obtained during the course of the expedition of the Duc de Luynes to the Dead Sea. M. Hébert contributes a paper "On the Lignite-bearing grit of Helsingborg and Höganäs in Southern Sweden."

WE have been requested to contradict the statement contained in our last number, that the *Journal of Botany* will in future be edited by Mr. Henry Trimen, of the British Museum. Dr. Seemann will continue to edit the journal; but greater prominence than hitherto will be given to British Botany, and that department will be under the joint superintendence of Mr. Trimen and Mr. Baker, of Kew.

A SPECIAL JOURNAL for the publication and discussion of observations of shooting-stars and bolidæ is about to appear under the editorship of M. Kieselmeyer, of Dresden. It will be published at irregular intervals dependent upon the amount of material in the hands of the editor. The price will be from 2fr. 50c. to 1fr., according to the number of subscribers.

THE culture of the Cinchona, or Peruvian bark, in St. Helena is progressing satisfactorily. The plants are all in excellent health, and have a fine, green, vigorous appearance. There are now about 4000 planted out, and it is thought a sufficient number can be obtained from them to stock the whole colony.

THE Southampton Town Council have decided to adopt the "A B C" process for the utilisation of sewage, and to make such arrangements with the Native Guano Company as may be agreed upon.

AUTHENTIC cases of the successful treatment of snake bites are of some interest. Dr. Bell supplies two in his "New Tracks in North America." On the Rio Grande, in October, 1867, two horses were bitten by the same rattlesnake, while grazing. A few hours afterwards the submaxillary, parotid, and all glands situated about the head and neck were greatly enlarged; from the nostrils and gums, a clear, mucous discharge ran down; the eyes were glairy, with the pupils greatly dilated, and the coat was rough and staring. To each animal Dr. Bell gave half-a-pint of whisky, with a little water, and half an ounce of ammonia, while the wounds were fomented with a strong infusion of tobacco, and afterwards poulticed with chopped tobacco leaves. Both horses recovered. One, although reduced in flesh, and thrown out of condition, was fit for work in a week, but the other only just escaped with his life, becoming a perfect skeleton, and only commencing to mend at the end of three months. Dr. Bell adds that a little weed, common throughout the Western States (called by Engelmann, *Euphorbia lata*, and by Torney, *E. dilatata*), is said to be a specific for the bite of the rattlesnake, but at the very time the plant was wanted it could not be found, although continually met with elsewhere, along the route, so that the experiment could not be tried.

PROFESSOR GIEBEL, of Halle, reports the results of some interesting experiments which he has made with the object of ascertaining the corr[illegible]s of the popular notion that sparrows are destructive anima[illegible], [illegible]ding chiefly on grapes and stone-fruit. He found on examining the intestines of seventy-three young sparrows, between the 18th of April and the 24th of June last, that forty-six of them had fed exclusively on insects (beetles, caterpillars, &c.), and seven only exclusively on stone-fruit, the rest having all more or less fed on insects. An examination of forty-six old sparrows gave similar results; three only were fruit-eaters and the rest chiefly insect-eaters.

A HAPPY idea, very ingeniously carried out, is the Pharmaceutical or Medico-Botanical Map of the World, produced by Mr. George Barber, of Liverpool. In a very clearly printed and carefully-coloured map we are shown at a glance the *habitats* of all the medicinal plants and drugs in general use, as well as the mean annual temperature of the countries whence they are obtained. The map is published by Simpkin, Marshall, and Co.

M. E. LIPPMANN has communicated to the *Annales Industrielles* an account of the operations for sinking the Artesian Well at La Chapelle. The engineers employed by the authorities of the city of Paris to execute this great work, are MM. Degosée, Laurent, and Co. When completed, this well will supply water to one of the most populous quarters of Paris. It is intended that the well shall not only strike the water-bearing stratum—at a depth of about 2,000 feet—into which the great well at Passy penetrates, but shall extend through the stratum to a total depth of 2,950 feet. In this way, other water-bearing layers will be intersected. The work was at first commenced by the ordinary method of sinking a masonry shaft 2 metres (6ft. 3⅝in.) in diameter to a depth of 445 feet through the tertiary strata which lie above the chalk. Many difficulties presented themselves, chiefly due to the looseness of the earth through which the excavation penetrated, and to the insufficient pumping-power. After two years of persistent labour, it was decided to try another system. At this period the shaft had reached a depth of 113 feet; new boring machinery driven by steam power was now set up, and until the present time the work has proceeded most satisfactorily.

ASTRONOMY

Prizes for the Discovery of Comets

THE following circular has been issued by the Imperial Academy of Sciences at Vienna:—

For several years past there have been remarkably few discoveries of new comets. The cause of this fact, which seems inconsistent with that of the wider distribution of telescopes suitable for such discoveries, may be due to the special attention that has been given to the small planets. It is, however, much to be regretted that there has been such slight increase in our knowledge of the comets, in view of the recently established connection between the shooting stars and the comets. It is exceedingly desirable that we should know more than two or three hundred out of the many thousands of comets which undoubtedly belong to our system, especially as most of those which we know move in parabolic orbits. Were our knowledge of comets more complete we should surely know of more meteor streams and comets belonging one to the other. Mindful of Herr Schumacher's words "it is natural that astronomers intrusted with the administration of a well furnished observatory should have no time left for sweeping the sky so minutely and so perseveringly as is necessary for discovering these faint bodies, whilst, on the contrary, it seems certain that to the many amateur astronomers who have less extensive means of observation hardly any more useful kind of activity could be recommended," the Imperial Academy of Sciences at Vienna is induced to propose for the discovery of comets during the three years from May 31, 1869, to May 31, 1872 eight prizes annually, consisting, as the receiver may choose, of a gold medal, or of twenty Austrian ducats representing its value in money.

The award of these prizes will be subject to the following regulations:—

1. The prize will be given only for the first eight comets discovered in each of the three years named, and only for such comets as are telescopic at time of discovery, that is, invisible to the naked eye. The comet must not have been before seen by another observer, and must be one whose appearance could not be predicted with certainty.

2. The discovery must be communicated immediately and without waiting for further observations, to the Imperial Academy of Sciences, by telegraph if practicable; and otherwise by the earliest post. The Academy undertakes to transmit the news immediately to other observatories.

3. The time and place of discovery with the plan and course of the comet must be given as exactly as possible with the first notice. This first notice is to be supplemented by such later observations as may be made.

4. If the discovery should be confirmed by other observers, the prize will not be awarded unless the observations of the discoverer suffice for the determination of the orbit.

5. The prizes will be awarded in the general meeting of the Academy held at the end of May of each year. In case the first notice of a discovery arrives between the 1st of January and the end of May, the final award of the prize will be deferred till the general meeting in May in the following year.

6. Application for the prize must be made to the Academy within five months from the time of the arrival of the first notice. Later applications will not be considered.

7. The Imperial Academy will procure the decision of the permanent astronomers of the Observatory at Vienna as to the fulfilment of the conditions in Nos. 1, 3, and 4.

The New Planet (109)

IN the *Astronomische Nachrichten*, 1779-80, there is a long and important article on Piazzi's observations by M. Argelander; also the approximate place of this planet, discovered by Prof. Peters of Clinton, New York.

The following elements of the Planet are by Prof. Peters, and are considered by him to be nearly accurate. We print in a parallel column the elements for the same planet as furnished by Prof. Axel Möller, of Lund. The latter are calculated from observations taken at Clinton on Oct. 13th, Leipzig Nov. 8th, and Lund Nov. 26th, 1869:—

Epoch 1869, Oct. 0·0 Berlin mean time.

M_0 =	337° 1′ 3·35″	350° 53′ 28·6″
π =	55° 53′ 48·0″	
☊ =	4° 51′ 45·4″	4° 57′ 30·
ι =	7° 56′ 56·55″	8° 3′ 57·8″
ϕ =	17° 25′ 14·13″	17° 27′ 51·0″
μ =	809″580	800″476
log a =	0·4278314.	

CHEMISTRY

Transformation of Chlorinated into Iodated Compounds

AD. LIEBEN has made important experiments on this kind of transformation. Ethyl chloride, mixed in a sealed tube with three or four times its weight of strong hydriodic acid, sp. gr. 1·9, and heated for five hours to 130°, is almost wholly converted into ethyl iodide, according to the equation—

$$C_2H_5Cl + HI = C_2H_5I + HCl.$$

In like manner ethylated ethyl chloride (butyl chloride), and amyl chloride are converted by strong hydriodic acid into the corresponding iodides, without formation of secondary products. Ethyl-chlorinated ethyl oxide $\left.\begin{matrix}C_2H_3(C_2H_5)Cl \\ C_2H_5\end{matrix}\right\} O$ is converted, by an excess of strong hydriodic acid, chiefly into ethyl iodide, and ethylated ethyl iodide (butyl iodide); but there are also some secondary products formed, viz. butyl chloride, alcohols, and a substance having a carbonaceous aspect, the quantity of these secondary products increasing as the hydriodic acid is less concentrated and present in smaller quantity.

To determine whether the action of hydriodic acid is a simple double decomposition or a case of the action of masses, the converse reaction was tried by heating ethyl iodide with a considerable excess of hydrochloric acid in a sealed tube to 130°. A small quantity of ethyl-chloride was thereby obtained, together with hydriodic acid and free iodine, showing that the inverse of the first-described reaction does really take place; but the quantity of ethyl chloride, which it yields, is very small, even when the action is continued for 50 hours. The result of the two supplementary experiments, namely, the decomposition of ethyl chloride by hydrogen iodide, and of ethyl iodide by hydrogen chloride, may be represented, though somewhat crudely, in the following manner—

$$\text{(Affinity of H for I)} + \text{(Affinity of } C_2H_5 \text{ for Cl)}$$
$$< \text{(Affinity of H for Cl)} + \text{(Affinity of } C_2H_5 \text{ for I)}.$$

The decomposition of ethyl chloride, and its homologues by the action of hydriodic acid, is analogous to the decomposition of silver iodide by the same reagent.

As an example of the action of hydriodic acid on organic chlorides of other series, *chloroform* $CHCl_3$ was introduced, together with 11 times its weight of hydriodic acid of sp. gr. 1·9,

into a sealed tube, and heated for 7 hours to 125°. The principal results of this reaction were hydrochloric acid, free iodine, and methylene iodide CH_2I_2. Now, remembering the fact, demonstrated by Kekulé, that iodides, submitted to the action of hydriodic acid, undergo an inverse substitution, the reaction just described may be explained by supposing that the chloroform is in the first instance converted into iodoform, which is then converted into methylene iodide by the action of the hydriodic acid, thus—

$$CHCl_3 + 3HI = CHI_3 + 3HCl,$$
$$\text{and } CHI_3 + HI = CH_2I_2 + I_2.$$

In other cases, the action represented by the second equation goes so far as to remove all the iodine from the iodated product formed in the first instance, and convert it into the corresponding hydride. Such, indeed, is the case with compounds belonging to the aromatic series. Berthelot [Bull. Soc. Chim. (2) ix. 30] has shown that Julin's chloride of carbon, or perchlorinated benzene, C_6Cl_6, is converted into benzene, C_6H_6, when heated to 280° with a large excess of hydriodic acid; and Lieben finds that monochlorobenzene, C_6H_5Cl, heated to 235°, for 15 hours, with from three to five times its weight of hydriodic acid, likewise yields benzene.

The action of hydriodic acid on organic chlorine-compounds appears, then, to exhibit two cases:—(1.) The chloride is easily converted into the corresponding iodide by double decomposition, whereas the transformation of the iodide into the corresponding hydride is difficult, and takes place only at high temperatures. In this case, if the experiment is well conducted, an iodide is obtained without a trace of hydride. Such is the case in the action of hydriodic acid on the chlorides of the series $C_nH_{2n+1}Cl$. (2.) The chloride is attacked by hydriodic acid with difficulty, and only at a high temperature, whereas the conversion of the iodide into hydride takes place easily, and at a comparatively low temperature. In such cases, as with the chlorides of the aromatic series, the product of the reaction is a hydride without any trace of iodide. In some cases, as that of chloroform, intermediate products are obtained, only part of the iodine being removed by the inverse reaction. [Giornale di Scienze di Palermo, v. 130.]

SCIENTIFIC SERIALS

THE *Archives des Sciences Physiques et Naturelles* for December 15, contains a paper by Professor Heer, on the Miocene Flora of Spitzbergen. The writer gives a preliminary account of the fossil plants collected and sent to him by the Swedish Polar Expedition of 1868. The number of species found in the Spitzbergen Archipelago amounted to 131, of which 123 were phanerogamic, and 8 cryptogamic. Figures and a detailed description of these are promised to appear in the Memoirs of the Stockholm Academy. The next paper is an extract of Thomsen's Thermochemical Researches (taken from Poggendorff's *Annalen*), to which Marignac has appended some valuable comments. Prof. Marignac adds a paper of his own, on the influence of water on the double decompositions of salts, and on the thermal phenomena which accompany them. The author was induced to publish this preliminary memoir in consequence of the appearance of Thomsen's results. He points out some interesting cases of retardation of chemical equilibrium, and intends to investigate them further. The rest of this number—the last of the year—is occupied by the usual *Bulletin Scientifique*, Meteorological Observations, and an index to the volume (xxxvi. N. S.)

In Polli's *Annali di Chimica applicata alla Medicina* for December, we observe, among other papers, a note on the action of hydric sulphate on iodides, by Dr. Vitali. It is generally supposed that the action referred to terminates with the production of sulphurous oxide and iodine; but Vitali has noticed, in addition, the formation of hydric sulphide and sulphur—fresh instances, consequently, of the reducing energy of hydric iodide. In a paper on Ferric Albuminate, Peretti shows that albumen is capable of dissolving Ferric oxide. The filtered solution, if evaporated at a gentle heat, dries up to a rose-coloured pellicle, which can be again dissolved in water, and coagulates at 75°. Details are given as to some of the reactions of the solution. Bellini contributes an article on the therapeutic (pharmaceutical) formulæ of sulphur. There are also several papers on dietetics, hygiene, and pathology, &c., taken from other journals, and an index to the volume, of which the present is the concluding number.

THE last two numbers of the *Revue des Cours Scientifiques* (Dec. 25, 1869, and Jan. 1, 1870), contain an elaborate paper on Vaccination, by M. Brouardel; a translation of Dr. Bence Jones's Croonian Lectures; an account of Schimper's Researches in Vegetable Palæontology, by M. Ch. Grad; and a lecture given by M. Bouchardat at the Paris Academy of Medicine "On the Mortality of Foster-children."

SOCIETIES AND ACADEMIES

LONDON

Geological Society, December 22, 1869.—Prof. Huxley, LL.D., F.R.S., in the chair. Messrs. Hopkinson, J. Sanders, and Jabez Church, C.E., were elected Fellows of the Society. The following communications were read:—1. "On the Iron-ores associated with the Basalts of the North-east of Ireland." By Mr. Ralph Tate, Assoc. Linn. Soc., F.G.S., and John S. Holden, M.D., F.G.S. The authors introduced their account of the iron-ores of the Antrim basalts, by stating that since 1790 an iron band had been known in the midst of the basalt of the Giant's Causeway, but that only within the last few years have further discoveries been made, which have developed a new branch of industry in the north-east of Ireland. The iron-ore of the numerous exposures was considered to represent portions of one sheet extending uniformly throughout the basalt and over a very large area. Indeed everywhere the iron band and its associated rock-masses present identical features, from which the authors deduced the following generalised section:—The underlying basalt gradually passes upwards into a variegated lithomarge of about 30 feet thick, graduating insensibly into a red or yellow ochre or bole of about 5-6 feet in thickness, which passes into a dense red ochreous mass of about 2 feet, charged with ferruginous spheroids consisting chiefly of a protoxide and peroxide. The spheroids are of the average size of peas; they increase in number and size towards the upper part of the band, and not unfrequently constitute that portion of it. The line of junction between the iron band and the overlying, and usually more or less columnar basalt, is in all cases well defined, and in a few instances exhibits decided unconformability. The authors discussed the several theories that may be suggested to account for the origin of the present condition of the pisolitic ore, and proceeded to point out what appear to have been the several stages of metamorphic action by which the pisolitic ore had been elaborated out of basalt. From field observations and chemical analysis, they have been led to consider the bole and lithomarge as the resultants of aqueous action in combination with acidulated gases, which, dissolving out certain mineral substances, has effected the decomposition of the basalts; and to assume that the bole underlying the iron band was a wet terrestrial surface, and that the subsequent outflow of basalt effected, by its heat, pressure, and evolved gases, a reduction of the contained oxides of iron into the more concentrated form in which they occur in the pisolite, the aggregation of the ferruginous particles being a result of the same actions. The ferruginous series, with interstratified plant-beds at Ballypalidy, was next described, and demonstrated to be of sedimentary origin; the ferruginous conglomerate resulting from the degradation of the pisolitic ore, of which it is chiefly reconstructed, and of the underlying ochres. Many additions were made to the list of plant remains from these beds; and priority of discovery of plants in the Antrim basalts was accorded to Dr. Bryce, F.G.S. Mr. D. Forbes was not prepared to admit some of the theoretical conclusions of the authors, and objected to calling in metamorphism to account for all that was hard to be understood. He could not recognise the division of beds so similar in character into two classes. He wished to know, assuming that the iron-ore merely resulted from the decomposition of the basalt, what became of all the silica and alumina which constituted three-fourths of the mass. The origin of the pisolitic ores was in fact organic. In Sweden certain lakes were regularly dredged each year for the pisolitic ore still in course of formation by means of confervoid algæ. He therefore regarded the whole of these beds as in a certain sense sedimentary, and though due to organic agency, yet still deriving their original mineral matter indirectly from the basalt. The basalt contained a considerable amount both of phosphorus and sulphur; and if the ores had been derived directly from the basalt, both these substances would have been present in them. This was an argument against any direct metamorphism. The presence of venadium afforded additional reasons for regarding these ores as formed in the same manner as bog iron and

similar ores. Sir Charles Lyell had observed in the basalts of Madeira red ochreous bands, which represented old land surfaces, in one of which Mr. Hartog and he had discovered a leaf-bed containing vegetation of much the same character as that of the island at the present day. Near Catania, in a recent lava-stream, he had seen the junction of the lava with the soil of the ancient gardens; and in character the soil now under the lava resembled the red beds in Madeira. Mr. W. W. Smyth was on the whole inclined to admit the power of metamorphism to produce such changes as had been here effected. He commented on the advantages of employing this Irish ore for admixture with hæmatitic ore, on account of the abundance of alumina present. Possibly there had been some difference in the chemical character of the different flows of basalt. Mr. Evans suggested that the Ballypalidy beds might be the littoral deposits of a lake in which the pisolitic ores of the other parts of Antrim were deposited farther from the shore, and subsequently buried under a basaltic flow. Mr. Etheridge inquired whether the pisolitic ore had been subjected to microscopic examination, with a view of finding traces of organic forms, such as *Gallionella*. Mr. Tate, in reply, defended his views as to metamorphic action. He thought the uniformity in thickness and character of the pisolitic ore band over so large an area showed that it could not be a lacustrine deposit. He had not as yet examined the spheroids under the microscope.

"Notes on the Structure of *Sigillaria*." By Principal Dawson, F.R.S., F.G.S., Montreal. In this paper the author criticised the statements of Mr. Carruthers on the structure of *Sigillaria* (see Q. J. G. S. xxv. p. 248). He remarked that *Sigillaria*, as evidenced by his specimens, is not coniferous; that the coniferous trunks found in the Coal-formation of Nova Scotia do not present discigerous tissue of the same type as that of *Sigillaria*; that no conifer has a slender woody axis surrounded by an enormously thick bark; that *Calamodendron* was probably a gymnosperm, and allied to *Sigillaria*; that although *Stigmaria* may not always show medullary rays, the distinct separation of the wood into wedges is an evidence of their having existed; that the difference in minute structure between *Sigillaria* and *Stigmaria* involves no serious difficulty if the former be regarded as allied to *Cycadaceæ*; and further, that we do not know how many of the *Stigmariæ* belong to *Sigillaria* proper, or *Favularia*, or to such forms as *Clathraria* and *Leioderma*, which may have been more nearly allied to *Lepidophloios*; that the fruit figured by Goldenberg as that of *Sigillaria* is more probably that of *Lepidophloios*, or may be a male catkin with pollen; and that he has found *Trigonocarpa* scattered round the trunks of *Sigillariæ*, and on the surface of the soil in which they grew. He agreed with Mr. Carruthers in regarding Mr. Binney's *Sigillaria vascularis* as allied to *Lepidodendron*. Prof. Morris thought that *Clathraria* and *Lepidophlois* ought to be discriminated from the *Sigillariæ*, as being rather more nearly allied with cycadaceous plants, especially the former. He pointed out the manner in which certain vascular bundles communicating between the centre of the stem of *Sigillaria* and allied genera and their bark might be mistaken for medullary rays.

"Note on some new Animal Remains from the Carboniferous and Devonian of Canada." By Principal Dawson, F.R.S., F.G.S., Montreal. The author described the characters presented by the lower jaw of an Amphibian, of which a cast had occurred in the coarse sandstone of the Coal-formation between Ragged Reef and the Joggins Coal-mine. It measured 6 inches in length; and its surface was marked on the lower and posterior part with a network of ridges enclosing rounded depressions. The anterior part of the jaw had contained about 16 teeth, some of which remained in the matrix. These were stout, conical, and blunt, with large pulp-cavities, and about 32 longitudinal striæ, corresponding to the same number of folds of dentine. The author stated that this jaw resembled most closely those of *Baphetes* and *Dendrerpeton*, but more especially the former. He regarded it as distinct from *Baphetes planiceps*, and proposed for it the name of *B. minor*. If distinct, this raises the number of species of Amphibia from the Coal-measures of Nova Scotia to nine. The author also noticed some insect remains found by him in slabs containing *Sphenophyllum*. They were referred by Mr. Scudder to the Blattariæ. From the Devonian beds of Gaspé the author stated that he had obtained a small species of *Cephalaspis*, the first yet detected in America. With it were spines of *Machairacanthus* and remains of some other fishes. At Gaspé he had also obtained a new species of *Psilophyton*, several trunks of *Prototaxites*, and a species of *Cyclostigma*. The President objected to the term *Reptiles* being applied to Amphibia, from which they were totally distinct. He questioned the safety of attributing the jaw to *Baphetes*, of which no lower jaw had been previously found. Mr. Etheridge remarked that the *Cephalaspis* differed materially in its proportions from any in either the Russian or British rocks.

"Note on a Crocodilian Skull from Kimmeridge Bay, Dorset." By J. W. Hulke, F.R.S., F.G.S. The author described a large Steneosaurian skull in the British Museum, from Kimmeridge Bay, which had been previously regarded as Pliosaurian, and was recently identified with *Dakosaurus* by Mr. Davies, sen. From the agreement of their dimensions, and their occurrence near together, the author thought it probable that this skull and the lower jaw described by him last session belonged to the same individual. It differs from the *Steneosaurus rostro-minor* in the greater stoutness of its snout, in the presence of an anterior pair of nasal bones prolonged into the nostril, and in the number of its teeth. The author proposed to name it *Steneosaurus Manseli*, after its discoverer.

"Note on some Teeth associated with two fragments of a Jaw, from Kimmeridge Bay." By J. W. Hulke, F.R.S., F.G.S. The author described some small teeth associated with fragments of a long slender snout not unlike that of an Ichthyosaur, but too incomplete to be certainly identified. The teeth are peculiar in the great development of the cementum, which gives the base of the tooth the form of a small bulb. The exserted crowns are slightly curved, smooth, cylindrical, and pointed. The attachment to the dentary bone was probably by means of the soft tissues, and the teeth seem to have been seated in an open groove in the surface of the jaw-bone. Until additional material reveals the true nature of this fossil, the author proposes to place it alone, and to call it provisionally *Euthekiodon*. The following specimens were exhibited:—Fossils and Rock-specimens from Antrim; exhibited by Ralph Tate, Esq., F.G.S. Fossils from Kimmeridge Bay; exhibited by J. W. Hulke, Esq., F.R.S.

Photographic Society, Dec. 14, 1869.—J. Glaisher, F.R.S., president, in the chair. The Secretary read a paper by Dr. Van Monckhoven, "On a new artificial light suitable for the production of photographic enlargements," of which we give the following abstract:—In M. Kirchhoff's analysis of the sun, he has shown that there are incandescent upon the sun's surface large quantities of calcium, sodium, iron, magnesium, chromium, &c. Whether these metals exist in a free state on the sun's surface, or whether they are in the form of volatile compounds, the presence of a very high temperature, *i.e.* combustion, would be sufficient to yield not only an extremely dazzling light, but also one possessed of considerable chemical power. These conditions actually exist in the sun, the chemical action of whose rays is due mainly to the presence of chromium, titanium,* and magnesium. The author has found by experiment that nearly all the metals of the alkalies and alkaline earths, as likewise many of the metalloids, when burning in oxygen, give rise to a large emission of chemical rays, due to the production of an oxide at a high temperature, and that the same phenomenon is evident when the same oxides are produced by the decomposition of the metallic salts in a volatilised condition at a very high temperature. Magnesium produces oxide of magnesium heated to whiteness by the flame. If we direct the jet of an oxyhydrogen lamp upon the carbonate or the chloride of magnesium, we produce in either case oxide of magnesium (magnesia) at a high temperature, and moreover obtain in both instances flames rich in chemical rays. So long as the salt is not entirely decomposed the light is sustained in all its brilliancy, but when nothing but magnesia remains the light loses its brightness, and at the same time the greater portion of its chemical activity. If metallic oxides (such as lime, magnesia, alumina, zirconia) are employed and heated by the oxyhydrogen flame to a very high temperature, the illumination is very brilliant; but it is much less photogenic in its character than when the oxide in a nascent condition is produced at a high temperature, as in the case of chlorides, carbonates, &c. In the latter instance the coloured lines of the spectrum inherent to each metal may be observed, but not in the former, and this circumstance induces me to believe that the chemical action of the sun is due to the cause mentioned. Magnesium is well known to emit an abundance of actinic rays; chromium is possessed of far greater chemical intensity. If dry hydrogen gas is passed through chlorochromic acid and afterwards ignited in a current of oxygen, oxide

* It is only recently that this metal has been discovered in the sun.

of chromium is produced at a very high temperature, and at the same time, a flame of such extraordinary chemical power, that chloride-of-silver paper held at a distance of twenty centimetres (eight inches) blackens sensibly in thirty seconds, or about as quickly as in full daylight. The same experiment may be conducted with equal success with chloride of titanium, which gives a blue flame of extraordinary chemical power. Unfortunately, these chlorides can be manipulated only by persons well versed in scientific research, as they become decomposed under the influence of moist gases, and the lamp then emits a considerable amount of vapour, as in the case of metallic magnesium. Magnesium, chromium, and titanium, all of which exist in the sun, are the sources of light most suitable for the purpose. The author is at the present moment occupied in establishing the coincidence of the ultra-violet rays of the spectrum with those of these metals. For the purposes of photographic enlargement, the author uses the Drummond system, substituting for the cylinder of lime, one of very pure carbonate of magnesia, free from soda, baryta, and iron, either alone in a very compressed state, or containing titanate of magnesia obtained by a mixture of chloride of titanium and carbonate of magnesia. The pillars are square at their base, three centimetres in diameter, and eight in height; they burn for an hour and a half, and cost less than half a franc apiece. They emit a very brilliant and economical light. Instead of pure hydrogen gas, ordinary coal gas, or even alcohol, together with oxygen, may be used. The preparation of oxygen, on the author's plan, is very easy, and free from danger. He employs for the purpose calcined oxide of manganese; it is then finely powdered and passed through a sieve. The chlorate of potash he uses is also pulverised and sifted; 600 grms. of brown manganese and 1,200 grms. of chlorate are well mixed by hand in an earthen vessel and sifted, care being taken not to allow any organic matter to enter, and the whole is then introduced into the wrought-iron retort A (fig. 1). The cork stopper E, covered with tinfoil, is put into its place, and the junction F,

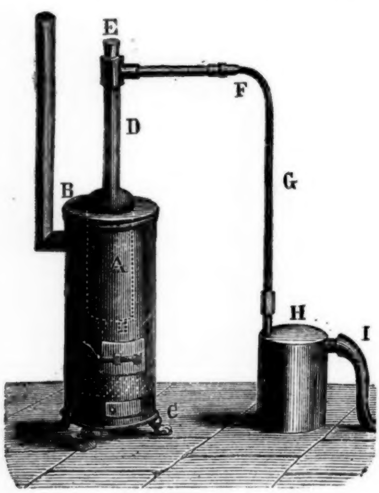


FIG. 1.

which places the retort, by means of the leaden pipe G and rubber tube I, in communication with the gas-bag, is adapted. The delivery tube (I) should be of at least half an internal diameter, and the wash-bottle H must be half filled with water. A small quantity of ignited charcoal is thrown into the little furnace B C, or a gas jet may be used, and after the lapse of a few minutes the india-rubber bag begins to inflate, and in twenty minutes it is full of oxygen; it is necessary during this operation to remove the weights and pressure-boards from the top of the bag. When the operation is finished and the retort somewhat cooled, the junction F is unscrewed, the cork E taken away, and warm water poured in until the retort A is filled. The water is allowed to remain for an hour, and the contents are then poured into a large jar, where, after the lapse of an hour or so, the oxide of manganese subsides. The clear water is decanted off, and the black deposit put upon a plate near the hearth to dry, after which it is again ready for repeated employment as often as desired. With ordinary native manganese a much higher temperature is necessary, the mixture having a tendency to puff up, and the operation becomes dangerous. For this reason it is advisable to use a cork stopper, E. A kilogramme of chlorate of potash yields 270 litres of oxygen, and this quantity will supply the lamp for two or three hours; thus the cost of our light, including coal-gas and the magnesia, amounts to two francs per hour. The oxyhydrogen burner used (shown in figs. 2 and 3) is very

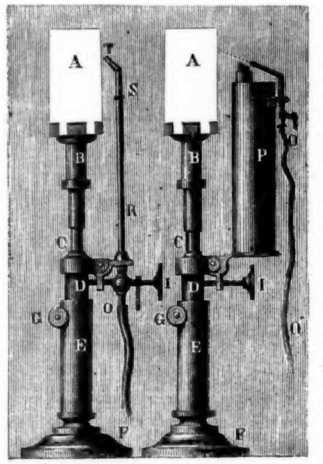


FIG. 2 FIG. 3

convenient. Those who have gas laid on in their houses will use the apparatus with two jets of gas (fig. 2); others will find it more expedient to employ the spirit-lamp arrangement (fig. 3). In both figures the same initials refer to similar details; A is the pillar of magnesia fixed upon a stem, B, which may be turned, lowered, or raised upon the rod C. E F is the stand or support, and G the pinion by means of which the light is adjusted in the centre of the apparatus. The jets for the two separate gases are formed by two concentric tubes, R, S T, sliding at S, so that the upper portion of the tube S T may be raised when it is desired to heat the top of the magnesia pillar A. Two stopcocks, O, P, lead the gases into the apparatus, the letters H and O being marked upon them to distinguish the oxygen supply-tube from that of the hydrogen or coal-gas. By means of a screw, I, the tubes R, S T, may be approached to, or removed from, the magnesia pillar. The coal-gas does not mix with the oxygen, excepting in the flame itself. The manner of employing the apparatus is exceedingly simple. The tube and stopcock marked H (connected with the supply of coal-gas), is first opened and the gas ignited; the stopcock marked O (in connection with the bag of

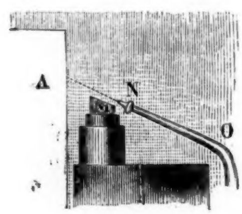


FIG. 4.

oxygen) is also opened, and the tube B then raised in such a manner that the top of the pillar A is heated by the flame, the extremity of the tube T being brought almost into contact with the magnesia. The heat soon indents the pillar, and *it is only when a cavity has been formed that the light attains its highest*

brilliancy; at this stage the stopcock H is partially closed until the maximum amount of light has been secured. The apparatus (fig. 3) is very similar to the other. The lamp P is filled with alcohol, the wick being round and cut obliquely, as in fig. 4, the extremity of the jet N O being near enough to the wick to touch it lightly. The wick M should be almost in contact with the pillar A, which is brought about by the screw I (fig. 3). To this apparatus there is connected but one india-rubber tube, O O', in communication with the oxygen bag. Fig. 4 shows the exact position that the jet N O should occupy in relation to the magnesia pillar. The enlarging-apparatus is shown in fig. 5. A case of

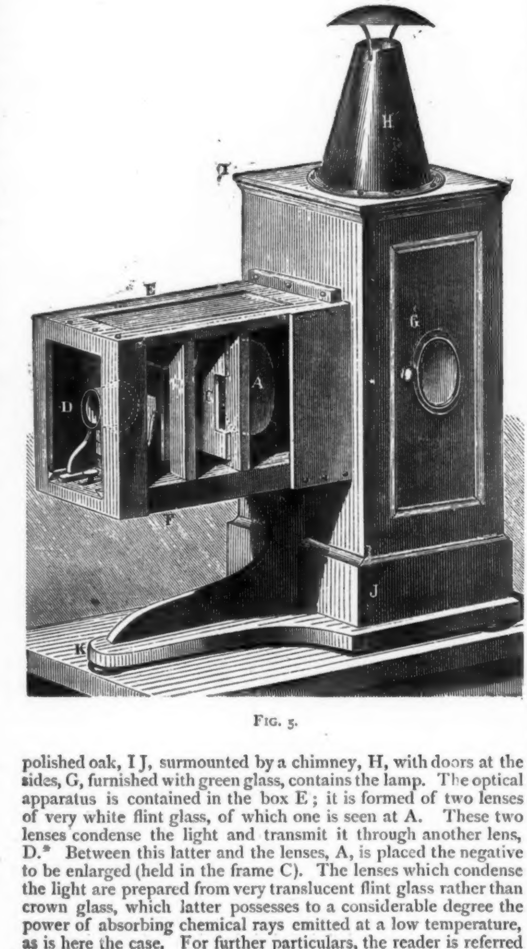


FIG. 5.

polished oak, I J, surmounted by a chimney, H, with doors at the sides, G, furnished with green glass, contains the lamp. The optical apparatus is contained in the box E; it is formed of two lenses of very white flint glass, of which one is seen at A. These two lenses condense the light and transmit it through another lens, D.* Between this latter and the lenses, A, is placed the negative to be enlarged (held in the frame C). The lenses which condense the light are prepared from very translucent flint glass rather than crown glass, which latter possesses to a considerable degree the power of absorbing chemical rays emitted at a low temperature, as is here the case. For further particulars, the reader is referred to the *Photographic Journal*, No. 212.

MANCHESTER

Literary and Philosophical Society, December 14, 1869.—J. P. Joule, LL.D., F.R.S., &c., president, in the chair. Sir Charles Lyell, Bart., LL.D., D.C.L., F.R.S., &c., and Henry Clifton Sorby, F.R.S., F.G.S., were elected honorary members of the society. Mr. R. Routledge was elected an ordinary member.—Mr. W. Boyd Dawkins, F.R.S., exhibited a stone-hammer and rude splinters of flint, brought over by Mr. Bauerman from the turquoise mines of the promontory of Sinai. These mines were worked, according to the evidence of the hieroglyphic inscriptions on the rock, by the Egyptians from the third to the thirteenth of the dynasties mentioned by Manetho. The tools and flint flakes found there in and around the workings, exactly coincide with the grooves in the rock made in the excavation, and evidently have been blunted by such use. There was no evidence that metal of any kind was used in the work. Mr. Bauerman also satisfied himself that the hieroglyphs were cut with implements similar to those used in the mining. This discovery is very important, because it opens up the question as to what tools the Egyptians used in working their wonderful monuments of granite and syenite. If it were worth their while to conduct turquoise mining with flint flakes in the Sinaitic promontory, and if they used the same tools in the hieroglyphs that fix the date of these mines—and of this there can be no reasonable doubt—it is very probable that they employed the same means for the same end elsewhere, and that, to say the least, a part of their marvellously minute sculpture in Egypt has also been wrought with flint. There is no evidence that they were acquainted with the use of steel. Iron and bronze are not hard enough for the purpose.—"On the Hades, Throws, Shifts, &c., of the Metalliferous Veins of the North of England," by Mr. J. Curry, of Boltsburn, Eastgate, County of Durham. Communicated by E. W. Binney, F.R.S., F.G.S. The new views, contained in this paper, are embraced under the consideration that the hades, throws, shifts, &c., may have been chiefly accomplished by peculiar modes of depositing of the sediments, during the contemporaneous building of the veins and strata. These modes were minutely described and illustrated by diagrams, which are requisite to convey a clear conception of the processes.

Physical and Mathematical Section, December 7, 1869.—E. W. Binney, F.R.S., F.G.S., president of the section, in the chair. "On the Mean Monthly Temperature at Old Trafford, Manchester, 1861 to 1868, and also the Mean for the Twenty Years 1849 to 1868," by G. V. Vernon, F.R.A.S., F.M.S.

PARIS

Academy of Sciences, December 27, 1869.—M. H. Sainte-Claire Deville called attention to the *Annuaire du Bureau des Longitudes* for 1870, and indicated that it contained a series of observations on the densities and co-efficients of dilatations of bodies which would render it useful in chemical laboratories.—M. C. Sainte-Claire Deville, in presenting a portion of the *Bulletin Météorologique de l'Observatoire de Montsouris*, noticed the progress of that establishment and the steps that are being taken for the cultivation of meteorology in France.—General Morin made a communication on some successful experiments which have been made on the acclimatisation of the *Cinchona officinalis* in the island of Réunion.—An extract from a letter of M. I. Pierre to M. Peligot, on the presence of potash and soda in various parts of plants, was read, in which the author stated that from his investigation of wheat, it appeared that where salt exists in the soil, both soda and potash occur in the plants grown on it, but that the latter increases in quantity up to the ear, whilst the soda is found especially in the lower parts of the plant. The amount of potash in different parts of the plant is in harmony with the amounts of nitrogen and phosphoric acid.—A note by M. S. Cloëz, on the disinfection of commercial sulphide of carbon, was presented. His process consisted in agitating the crude substance with $\frac{1}{2}$ per cent. of its weight of finely-powdered corrosive sublimate, which throws down a semifluid compound of disagreeable odour. The supernatant liquid is then decanted and distilled. — Some chemical researches on copper, by Mr. T. Sterry Hunt, were communicated. The author referred to the resemblances existing between perchloride of silver and protochloride of copper, which also extend to the oxides. He described the behaviour of protoxide of copper with various chlorides, especially those of magnesium and iron, and also that of peroxide of copper with protochloride of iron.—M. Daubrée presented a note by M. Terreil on the modifications undergone by minerals by the action of saline solutions. It related to the action of the alkaline monosulphides upon the natural metallic sulphides, selenides, and tellurides, singly or in combination.—A note by M. J. Personne, on the preparation and properties of hydrate of chloral, was read. The author described the differences in the physical properties of the substances obtained by himself and M. Roussin as hydrate of chloral, and indicated that these are due to the fact that the

* In apparatus where the lenses, A, are ten inches in diameter, one of them may be removed, and the case, I J, also; then, by adjusting a reflector, it is possible to work by sunlight when the same is procurable.

compound prepared by the latter is not pure, but contaminated by the presence of a considerable amount of alcohol.—M. Dubrunfant continued the discussion on the nature of inverted sugar, by a description of his method of separating levulose fr m it. He effects the separation by the addition of hydrate of lime to a solution of inverted sugar, presses the crystalline magma produced, and removes the lime by treating both the solid residue and the expressed fluid with an acid. This process, according to the author, effects the nearly complete separation of the two forms of glucose, and he suggested that it might become of importance, as levulose possesses far higher sweetening powers than right glucose.—Mr. T. L. Phipson communicated a note on some substances extracted from the fruit of the walnut. From the green envelope of the fruit he obtained a yellow, crystalline substance, of little stability; this, in a few hours, produced a black, amorphous, resinous substance, $C_6 H_6 O_7$, which the author called *regianic acid*. With alkalies it forms soluble salts of a magnificent purple colour, and with oxide of lead a violet-brown insoluble salt. For the yellow body he proposed the name of *regianine*. A substance occurring in the episperm of the nut was called *nucitannine*; it is the cause of the harsh taste of that skin. From it, by treatment with mineral acids, the author obtained glucose, ellagic acid, and a red, insoluble body, which he named *rothic acid*. Its composition was said to be $C_{28} H_{12} O_{14}$. The green envelope, when fresh, absorbs oxygen with avidity from the air; when mixed with soda, it absorbs oxygen much more rapidly than phosphorus.—In a note on the simultaneous action of the intra-pilar current and nascent hydrogen upon organic acids, M. E. Royer described his treatment of oxalic acids by these agencies. Concentrated solution of that acid, placed in the porous cell of a Grove's battery, furnished a considerable quantity of formic acid in a few days, the oxalic acid having been split, and hydrogen having combined with each of the two half-molecules. No carbonic acid was set free.—M. Delafosse presented a report upon M. Kokscharow's contributions to the mineralogy of Russia, indicating the general character of that work.—M. Feil exhibited some specimens of heavy glass (Faraday's glass), prepared by a new process which enables it to be produced in large masses. He also sent in some examples of artificial gems.—A note by M. M. A. Gaudin, on the production of artificial gems, was also communicated; it was accompanied by a small collection of specimens.—A memoir was presented on the general movements of the atmosphere, by M. Peslin; also one on the graduation of galvanometers, by M. P. Blaserna; and another, containing the first part of a new method for the solution of problems in mechanics, by M. Piaron de Mondesir.—Of biological papers, M. Lacaze-Duthiers communicated a first memoir on the morphology of the mollusca, relating to the *Gasteropoda*. To this we may probably refer elsewhere.—M. P. P. Dehérain presented a paper on the metamorphoses and migrations of the proximate principles in herbaceous plants, in which the author traced the course of the more important vegetable compounds from one set of organs to another during the life of the plant, and indicated the changes which they undergo in different parts. He ascribed the transport of soluble materials from one part of a plant to another to the varying amount of aqueous evaporation from the surface. The accumulation of insoluble proximate principles in the seed was also accounted for by the author on the supposition (experimentally arrived at) that wherever in a system fully charged with liquids there is a point at which the dissolved elements become insoluble, they tend towards that point in order to maintain the equilibrium. Of the means by which the soluble elements are converted into insoluble ones, the author attempted no explanation.—M. Milne Edwards presented a note by M. Balbiani on the constitution and mode of formation of the ovum in the *Sacculinæ*, in which that author contests some of the points insisted on by M. E. van Beneden in a former paper (see NATURE, p. 246).—The question of the antiquity of the horse in Egypt formed the subject of notes by MM. F. Hément, F. Lenormant, and Faye. M. Lenormant disposes of the passage in Genesis in which *mules* are supposed to be referred to. He seems inclined to consider that the word translated mules (which occurs nowhere else in the Bible) really signifies hot springs. M. Faye, in opposition to all authority, holds fast by the ordinary modern version, and also cites the passage in the same book in which horses are mentioned among the animals taken by Joseph in exchange for corn during the years of famine in Egypt. From the fact that horses are here familiarly mentioned, M. Faye infers that their employment in Egypt as domestic animals must then have been of long standing.—M. E. Decaisne communicated a paper on suckling by mothers; and Mr. T. L. Phipson a note on the explosion and fall of meteorites. Papers were also presented by M. Bonjean, on the detection of hydrocyanic acid and cyanides in cases of poisoning; by M. Guyot, on the toxical effect of rosolic acid; by M. Trouvé, on the employment of electricity in seeking metallic bodies in wounds, &c.; by M. L. Colin, on telluric emanations and their connection with fevers; by M. Gouteyron, on the influence of the shell of iron vessels upon the compass; by M. Jouglet, on the production of an explosive powder by the action of coal-gas upon nitrate of copper; and by M. Dupuis, on a new hydraulic lever.

DIARY

THURSDAY, JANUARY 6.

ROYAL SOCIETY, at 8.30.—Some Account of the Suez Canal: J. F. Bateman, F.R.S.—On the Mineral Constituents of Meteorites: N. Story Maskelyne.—On Fluoride of Silver: G. Gore, F.R.S.

ROYAL INSTITUTION, at 3.—On Light (Juvenile Lectures): Prof. Tyndall, F.R.S.

SATURDAY, JANUARY 8.

ROYAL INSTITUTION, at 3.—On Light (Juvenile Lectures): Prof. Tyndall F.R.S.

MONDAY, JANUARY 10.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

MEDICAL SOCIETY at 8.

TUESDAY, JANUARY 11.

CIVIL ENGINEERS, at 8.

PHOTOGRAPHIC SOCIETY, at 8.

ETHNOLOGICAL SOCIETY, at 8.—On the Kitai and Kara-Kitai: Dr. Gustave Oppert.—On the Origin of the Tasmanians, geologically considered: J. Bonwick, Esq.—On some Prehistoric Remains discovered in New Zealand: Dr. Julius Haast, F.R.S.

WEDNESDAY, JANUARY 12.

MICROSCOPICAL SOCIETY, at 8.—On the Calcareous Spicula of the Gorgoniadæ: W. S. Kent, F.Z.S.—On an Undescribed Stage of Development of Tetrarhyncus Corollatus: Alfred Sanders, M.R.C.S.—On a New Method of Measuring Spectra Bands: John Browning, F.R.A.S.

GEOLOGICAL SOCIETY, at 8.—On the Superficial Deposits of Portions of the Avon and Severn Valleys and Adjoining Districts: T. G. B. Lloyd, Esq., C.E., F.G.S.—On the Geological Position and Geographical Distribution of the Reptilian or Dolomitic Conglomerate of the Bristol Area: R. Etheridge, Esq., F.G.S.

THURSDAY, JANUARY 13.

LONDON MATHEMATICAL SOCIETY, at 8.—Equations of Centres and Foci of certain Involutions: Mr. J. J. Walker.

CONTENTS

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